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THE AMERICAN

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CURRENT NOTE.

We reproduce on the opposite page a photograph of a tornado which formed in the southwest corner of Anderson Co., Kansas, about 5 P. M. on April 26, 1884. The photograph was taken by Mr. A. A. Adams, of Garnett, at a distance of 14 miles. The tornado travelled north-northeast at the rate of about 50 miles per hour, and its total path was 20 to 25 miles long. The track was from 5 to 40 rods wide. The storm was very destructive to property, but no lives were lost. During the tornado a distinct upward motion was seen in objects in the spout. Soon after the photograph was taken the spout broke with a hissing sound like that made by water thrown on red-hot iron.

The photograph of the Howard tornado, which we mentioned in the December number, has been copyrighted and can not be reproduced here. It can be seen in a recent number of *Nature.*

DETERMINATION OF AIR TEMPERATURE AND HUMIDITY.

Two important articles on this subject have recently appeared in the *Zeitschrift für Meteorologie* (Austrian). The first by Dr. Assman, on the Thermometer "fronde," in April, has already been translated and published in the journal for July. The second by Prof. Wild is published in the *Zeitschrift* for October. Many of the principles set forth in the latter paper are believed to be founded on insufficient evidence and are contradicted by the results of careful and long continued experiment. It is my desire in this article to criticize, in a friendly spirit, some of Prof. Wild's assertions, and to set forth some of the more prominent facts determined by experiment.

It may be well at the outset to lay down certain general principles, regarding different radiations, universally accepted by meteorologists, as much confusion of thought will thus be avoided. The radiations the meteorologist deals with may be divided into four general classes:

1st. Solar radiation.

2d. Reflected heat, the same in kind as the 1st.

3d. Dark radiation or that from bodies not incandescent.

4th. Radiation into space.

The first two affect bright and black bulb thermometers unequally causing the black bulb to read ordinarily about as much higher than the bright as that reads higher than the air temperature. The 3d affects bright and black equally. The necessity of a clear apprehension of the above points is best illustrated by the advocacy in certain quarters of the use of black and bright bulb thermometers in determining the amount of dark radiation from the sides of a shelter. The use of the black bulb thermometer in ascertaining the effect of direct solar radiation and of reflected heat, is important in investigating the proper method of exposing thermometers, and should be strongly emphasized.

The necessity of an absolute standard of air temperature for comparison with temperatures obtained under varying conditions will be readily recognized. The following method may be regarded as approaching more nearly such standard than any yet proposed. Upon a large, grassy plain, with no buildings near, we shall be rid of nearly all effects of radiation, except those directly from the sun, and in consequence we shall be obliged to provide against that

only. Since there seems to be a nearly constant relation existing between the indications of black and bright bulb thermometers and the air temperature when affected only by solar radiation, we need only to use these thermometers in conjunction and after finding the relation existing between the three assume that as constant.

The following notation will be used for convenience:

Let t_a = air temperature; t_{br} = bright bulb temperature; t_{bl}=black bulb temperature.

We have then $t_a = t_{br} - c\ (t_{bl} - t_{br})$, or the air temperature is equal to the bright bulb temperature minus the difference between the black and bright bulbs multiplied by a constant which is nearly equal to unity. The following experiments have been tried since the adoption of this standard. These were made in November with more or less wind blowing and are given only as an approximation of what may be hoped with a larger number of observations under varying conditions. The difierent conditions were taken up forward and back to eliminate the effect of changes in temperature between the first and last observations, but much uncertainty exists because the readings were not simultaneous.

Three conditions were employed: 1st. Under an umbrella in full sunshine; 2nd. In bright sun; 3d. In the shade of a small barn. Under each of these conditions a black and bright bulb thermometer lashed together were read both still and in rapid motion by swinging. In order to ascertain the effects of friction at high velocities the device was at two different times swung inside the barn at velocities varying between 3 and 14 meters per second (7 and 31 miles per hour) with identical results.

The following table gives values from the mean of 8 or more individual observations.

Umbrella.				Sun.				Shade.			
Still.		Swung.		Still.		Swung.		Still.		Swung.	
t_{bl}	t_{br}	t_{bl}	t_{br}	t_{bl}	t_{br}	t_{bl}	t_{br}	t_{bl}	t_{br}	t_{bl}	t_{br}
44.9	44.2	44.1	43.6	50.7	46.4	45.2	44.0	43.3	42.8	42.7	42.5
$t_a.c$=1	43.5		43.1		42.1		42.8		42.3		42.3

Assuming c as unity we have the results in the 5th line, of these it seems probable that those in columns 3 and 4 are nearest the true air temperature, and taking the value of c as unity in this

case, we may determine values of c for the other cases, *e. g.*, umbrella, still, $c = 1.59$; sun, still, $c = .75$; sun, swung, $c = .77$ In the shade the effects of solar radiation are entirely cut off though reflected heat is very slightly felt; at the same time the effects of radiation into space are at a maximum, and in consequence the temperature obtained is about .8° too low. This investigation while not absolutely confirming the value of c, as unity with the black and bright bulb "fronde" under an umbrella, the latter in full sunshine, yet seems to show that this method will give a better air temperature than any yet devised.

Two general propositions have been advanced regarding the proper exposure of thermometers:

1st. That the most essential characteristic is thorough, natural and artificial ventilation.

2nd. That natural ventilation may be partly dispensed with in shielding from radiation and rain.

The best example of the 1st class is the "psychrometer fronde" ably advocated by Dr. Assman. His investigations run parallel with, and agree almost exactly with those made independently in this country and published in *American Journal of Science* for May of this year. Minor exceptions to the principles stated by Dr. Assman, and to the results of a few experiments, may be taken as follows:

1st. Dr. Assman thinks that all radiation and rain should be cut off. It may be said that the effects of all radiation should be overcome as much as possible, but, if in shielding from harmful effects too little natural ventilation is allowed, the results will be unsatisfactory.

2nd. Dr. Assman would swing the psychrometer in a shady place if the sun shines. As already shown the dense shade of a building is objectionable because of great radiation into space and a consequent fall in temperature below the t_a, but if the shade of a tree with not too dense foliage be chosen, it will give a good result, and with a wind almost any shade will be satisfactory.

3d. Dr. Assman found with velocity of 3^m per second a relative humidity 3 per cent. less than with a slow pendulum motion. It seems probable that in the latter case a slightly lower reading would have been had after a longer interval of swinging and after more of the water had evaporated from the wet bulb.

4th. Dr. Assman gives 25 seconds as the time required to bring down the wet bulb at a velocity of 6^m per second. On a fresh

wetting the amount of time required is a little more than 25 seconds, generally it is best to swing 50 times, then 25 times and then 25 times more, reading after each set; unless the air is very dry and the temperature below 32° there will be found little or no change between the last two sets. If the temperature of the wet bulb is below freezing it will frequently be found at exactly 32° after one or two sets, and this too even though it had read several degrees lower just previous; the upward motion of the mercury is rapid and is due to a sudden formation of ice. It will be found that the smaller the thermometer bulb the more quickly will ice form. Under these conditions, often requiring a delay of more than an hour when the psychrometer is exposed without ventilation, it will be necessary to swing for a minute or more before the lowest reading can be had.

Prof. Wild in his recent paper has attacked this very simple device as follows: On one occasion, because of radiation into space coupled with dark radiation, a stationary thermometer in a tin shelter in the open air on a clear calm night read 1.6° lower than one in his shelter, to be shortly described. Prof. Wild seems to infer from this that the thermometer "fronde" will also give much too low readings at night due to the same cause. It should be noted, however, that the conditions are not at all similar in the two cases. It is well known that the effect of radiation into space is largely diminished by a high wind, now this in effect is precisely what "fronde" does, it takes on the t_a because it rapidly passes through a mass of air and this imparts its temperature. The best proof of the inappreciable effect of radiation into space at night may be obtained by swinging the "fronde" under an umbrella, or an open shed or tent; in such case all radiation into space is cut off, and yet the same results will be found under such screen as in the open air.

Prof. Wild rather facetiously pictures an observer at night, in rainy weather, holding an umbrella, lantern, notebook and pencil in one hand, and using the "fronde" with the other. The feat is by no means as difficult as might be supposed. At a tenth of the expense of one of Prof. Wild's double shelters, a large umbrella (to be dropped when not in use) can be permanently placed upon a post, a nail upon another post will serve to hold the lantern, and a little shelf can be easily arranged for the dish of water, notebook and pencil. In climbing a mountain 3500 ft. high in December, 1883, the writer of this in addition to the articles mentioned above, excepting the umbrella, carried an extra psychrometer and an aneroid

barometer. Observations were made at vertical intervals of one to two hundred feet. At the summit the temperature was 7.3. F. below zero and yet there was no difficulty in placing the lantern on the ground; taking the bottle of water from an inside pocket (placed there to keep it from freezing) wetting, swinging and reading the psychrometer, then the aneroid carried upon a little shelf supported from the shoulder.

The experiments of Prof. Wild in testing "fronde" can hardly be regarded as conclusive. As just suggested the deep shade of a house cannot give the t_a, but will nearly always be below it. The shade of a large umbrella frequently would give a much more satisfactory result. Most careful experiments in the shade of tall trees and in the sun have shown "fronde" about .7° too high value in bright sunshine with no wind. No one, however, has advocated the use of "fronde" in direct sunshine.

While "fronde" gives by far the most correct value for t_a, yet it is especially in the use of this device for obtaining the humidity of the air, that its most important advantage appears. As a proof of the necessity of a better ventilation of the psychrometer, mention need only be made of the varying results and numerous psychrometric formulæ obtained by different experimenters.

M. Sworykin in St. Petersburg has published results of carefully conducted comparisons between a psychrometer in Prof. Wild's ventilated screen, and Schwakhofer's volume hygrometer as well as Alluard's condensing hygrometer. Taking the value of A in the ordinary psychrometric formula $A = \frac{f-x}{h(t-t')}$, we find, in 23 observations the values of A varying between .00188 and .00049.

Another short series of 12 observations, on Pike's Peak, in no one of which was the wet below freezing, gives a range from .00268 to .00031; comparing with "fronde" we find the latter in about 1000 observations giving A ranging from .00097 to .00049 at all temperatures from 23° to 90°. This constant for temperatures above 32° and at sea level has been determined as follows: Regnault, .00074; Sworykin, .000725; Blanford, .000827; Angot, from over 3000 observations, .000851; Chistoni, .000851; Doyere, in 1855 from 40 observations, with "fronde" above 32°, .000687; my own result from 1000 observations with "fronde" .000678. The last two values are nearly the same, and also agree closely with Sworykin made under favorable conditions. At temperatures below freezing the difficulty of obtaining a correct reading of the psychrometer as ordinarily used is increased many fold. Even with

ordinary means of artificial ventilation, the results have been very unsatisfactory. One of the best proofs of this is to be found in the discussion as to the effect of water or ice on the wet bulb just at freezing.

H. ALLEN HAZEN.

(*To be continued.*)

THE DIURNAL VARIATION OF WIND-DIRECTION AT COLORADO SPRINGS.

In a letter which appeared in the September number of this JOURNAL reference was made to a projected attempt to exhibit in a table the relation existing at this place between the hour of the day and the mean direction of the wind. Such a table is herewith presented; but, in order to state connectedly the materials from which it has been drawn and the method of reduction employed, it will be necessary to repeat a portion of the contents of the former letter.

The table is derived entirely from records of the wind's *direction*, without reference to *velocity*. The instrument employed was an anemoscope made on the plan of Dr. Draper's in the New York Central Park Observatory, and consists of a cylinder turning with the vane on a vertical axis, while a pencil traverses its length in twenty-four hours. The circumference of the cylinder, or breadth of the sheet of paper containing the daily register, is twelve inches; and the first step of the reduction consisted in applying to the register of each hour a foot-rule in which the inch-spaces were lettered *a*, *b*, *c*, etc. The first inch, *a*, embraced therefore 30° of the horizontal section of the cylinder, and extended from N. to N. 30° W.; and if it was decided that the registered wind of a particular hour fell within this portion of the horizon, the letter *a* was inserted in the proper place in a table ruled horizontally for the days of the month and vertically for the hours of the day.

In deciding to which of the twelve divisions the wind of a particular hour should be referred the rule adopted was, whenever the wind remained in one of the divisions for more than half of the hour, to record that division without regard to its place for the remainder of the time. When for several hours the wind oscillated

Hour Ending.	November, 1883.			December, 1883.			January, 1883.			February, 1884.		
	N. Comp.	W. Comp.	Mean Dir.	N. Comp.	W. Comp.	Mean Dir.	N. Comp.	W. Comp.	Mean Dir.	N. Comp.	W. Comp.	Mean Dir.
1 A. M.	+18.7	− 2.2	353°	+10.6	+ 1.9	10°	+16.7	+ 2.0	7°	− 0.5	− 6.3	265°
2 "	+17.7	− 6.4	340	+13.7	− 0.9	356	+17.3	+ 5.9	19	+ 4.6	− 3.4	324
3 "	+21.2	+ 0.5	1	+14.3	+ 3.9	15	+16.9	+ 6.6	21	+ 8.6	− 2.9	342
4 "	+21.7	+ 2.2	6	+14.4	+ 0.3	1	+17.3	+ 5.9	19	+11.5	− 1.0	355
5 "	+20.8	+ 3.1	8	+14.6	+ 6.2	23	+17.6	+ 6.0	19	+10.8	+ 4.1	21
6 "	+17.9	+ 4.7	15	+17.5	+ 4.0	13	+17.4	+ 3.0	10	+11.4	+ 3.3	16
7 "	+20.5	+ 5.7	16	+17.9	+ 0.7	2	+17.9	+ 6.6	20	+ 8.9	+ 4.1	25
8 "	+20.7	+ 4.1	11	+19.2	+ 2.0	6	+16.5	+ 0.5	2	+12.2	+ 7.2	30
9 "	+19.0	+ 5.1	15	+15.0	− 0.6	358	+12.6	+ 0.3	1	+ 8.2	+ 4.6	29
10 "	+10.5	+ 1.1	6	+13.3	+ 0.4	2	+15.5	+ 1.5	6	+ 7.3	+ 0.5	4
11 "	+ 1.5	− 8.3	281	+11 9	− 2.2	350	+11.9	− 3.5	343	+ 3.5	− 0.2	357
12 M.	− 8.1	− 6.8	220	+ 9.0	− 4 2	335	+ 9.8	− 1.8	350	+ 2.6	− 4.8	299
1 P. M.	−15.1	− 8.1	208	+ 5.2	− 3.5	326	+ 6.6	− 4.8	324	− 0.6	− 7.9	266
2 "	−13.1	−10.9	220	+ 8.4	− 4.6	331	+ 6.8	− 4.6	326	+ 0.1	− 3.4	271
3 "	−16.6	−14.1	220	+ 3.1	−10.6	286	0	−10.2	270	− 5.2	− 0.8	188
4 "	−17.6	−10.1	210	+ 2.7	−12.5	282	− 3.3	−11.8	255	− 1.5	− 6.1	256
5 "	−16.8	− 6.3	200	+ 3.4	− 9.6	289	− 2.4	−10.5	257	− 0.2	+ 0.1	150
6 "	− 8.9	− 3.3	201	+ 5.9	−12.0	296	− 1.9	−10.0	259	− 1.1	− 4.0	255
7 "	+ 2.1	− 1.2	330	+10.7	−10.3	316	+ 5.4	− 7.7	305	− 2.8	− 2.1	217
8 "	+ 9.1	− 0.5	357	+13.2	− 7.5	330	+ 8.5	− 2.4	344	− 4.6	− 3.6	218
9 "	+14.9	− 1.6	354	+12.1	− 6.2	333	+ 9.7	− 2.8	344	+ 8.0	+ 5.4	34
10 "	+18.9	− 1.3	356	+13.2	− 4.5	341	+13.3	− 1.4	354	+ 1.2	+ 0.5	21
11 "	+14.1	+ 3.2	13	+13.5	− 3.9	344	+14.0	+ 1.9	8	+ 3.2	− 6.3	297
12 "	+18.0	− 2.5	352	+ 9.1	− 2.4	345	+12.9	+ 1.2	5	+ 1.2	− 5.6	262

March, 1884.			April, 1884.			May, 1884.			June, 1884.			July, 1884.		
N. Comp.	W. Comp.	Mean Dir.	N. Comp.	W. Comp.	Mean Dir.	N. Comp.	W. Comp.	Mean Dir.	N. Comp.	W. Comp.	Mean Dir.	N. Comp.	W. Comp.	Mean Dir.
+ 7.9	− 1.2	352°	+ 7.6	− 5.4	324°	+12.7	− 3.7	344°	+ 1.3	− 2.9	294°	+13.7	− 0.5	358°
+ 8.6	− 1.9	347	+ 7.5	− 4.2	331	+15.3	− 2.9	349	+ 6.7	− 3.8	331	+13.8	+ 0.9	4
+10.3	+ 0.5	3	+ 7.0	− 2.3	342	+14.2	− 1.5	354	+ 3.4	− 4.3	308	+14.8	− 1.0	356
+ 9.1	+ 1.6	10	+ 7.7	− 4.9	327	+14.0	− 4.2	343	+ 8.9	− 4.5	333	+18.5	− 2.8	351
+12.4	− 3.6	344	+10.7	− 5.2	334	+15.2	− 6.2	338	+10.4	− 5.1	334	+18.9	− 2.1	354
+13.0	+ 2.3	10	+11.7	− 6.0	333	+14.4	− 4.9	341	+11.4	− 1.7	351	+19.2	− 2.6	352
+14.5	− 2.3	351	+11.6	− 4.4	339	+12.9	− 5.9	335	+ 8.7	− 4.1	335	+18.7	− 3.7	349
+14.8	− 1.3	355	+ 8.8	− 5.8	327	+11.9	− 6.2	332	+10.2	− 3.8	340	+15.5	− 4.8	343
+11.6	− 0.6	357	+ 5.5	− 5.0	317	+ 7.7	− 7.0	318	+ 4.8	− 3.6	323	+ 8.8	− 9.1	314
+ 8.1	− 2.0	346	+ 0.2	− 7.1	272	− 1.2	− 9.7	263	− 6.6	− 5.3	219	− 1.8	−10.5	260
− 1.8	− 5.1	251	− 0.1	− 6.1	269	− 9.0	− 8.1	222	−11.8	− 8.3	215	−10.4	−10.0	224
− 6.5	− 2.3	199	− 1.0	− 5.1	259	− 8.8	− 8.4	224	−16.8	−10.3	211	−13.9	−12.1	221
− 5.9	+ 0.8	172	− 0.8	− 4.7	260	− 8.4	− 8.4	225	−23.1	− 4.8	192	−19.6	− 9.5	206
− 4.4	− 2.7	212	− 1.5	− 5.2	254	− 5.1	−10.3	244	−20.1	− 4.6	193	−15.7	− 9.2	210
−10.2	− 5.6	209	− 0.5	− 3.7	263	− 7.5	− 7.0	223	−16.6	− 5.7	199	−12.2	− 5.9	206
−10.7	− 2.4	193	+ 1.4	− 1.1	321	− 3.7	− 7.4	244	− 7.9	+ 3.9	154	− 9.2	− 7.3	218
− 9.7	− 2.6	195	+ 2.9	− 4.6	302	+ 0.8	− 4.8	280	− 7.4	+ 1.7	167	− 8.2	− 6.8	220
− 8.9	+ 2.5	164	+ 1.5	− 1.1	325	− 0.4	− 8.5	267	− 7.5	− 2.4	197	− 5.9	− 7.7	233
− 7.4	− 1.2	188	− 0.5	− 3.1	261	+ 1.1	− 8.3	278	0	− 4.1	270	− 8.3	− 6.6	219
− 4.0	− 1.7	203	+ 2.6	− 1.3	333	+ 0.7	− 9.1	274	− 2.1	− 5.2	248	− 7.3	− 5.4	217
− 1.7	− 3.8	246	+ 4.7	− 1.9	339	+ 3.9	− 4.1	314	− 1.5	− 6.8	257	− 3.9	− 6.8	240
+ 0.7	− 7.3	276	+ 4.2	+ 0.6	9	+ 4.9	− 2.3	335	− 2.9	− 7.4	249	+ 4.1	− 2.3	331
+ 5.5	− 7.3	307	+ 5.9	− 3.6	328	+ 8.7	− 6.6	323	+ 0.7	− 6.9	276	+ 8.2	− 1.8	348
+ 4.2	− 2.6	328	+ 4.6	− 1.6	340	+ 7.3	− 6.8	317	+ 1.0	− 2.4	293	+12.2	− 1.3	354

between two adjacent divisions, the corresponding letters were inserted alternately; but when a marked change in the wind's direction took place about the middle of the hour, a zero was entered for that hour, unless the change was only one of about 60°. so as to pass, *e. g.*, from *f* to *h;* in which case the intermediate letter, *g*, was inserted, even though there had been no wind from that division during the hour. It was, in order to avoid inserting zeros too frequently, that divisions embracing so large an arc as 30° were adopted rather than smaller ones.

When the winds of a month had been tabulated as above, a new table was made, ruled one way for the hours of the day and transversely for the twelve divisions of the wind; and this table showed how many times any particular letter had been recorded in the previous table under any one hour of the day. Each number occurring in the second table was then resolved into a northerly and a westerly component, (by multiplication into the cosine and sine of the angle formed with the meridian by the line drawn to the middle point of the proper arc of thirty degrees), and the northerly and the westerly components of all the numbers belonging to each hour were added algebraically. The sum of all the numbers entered in the second table against a particular hour should be 29, 30, or 31, according to the number of days in the month, but on account of the insertion of zeros in the first table, as described, it was often less, and for the sake of uniformity, whenever this sum differed from 30, the sum of the northerly and that of the westerly components were each increased or diminished in the corresponding ratio, so as to make the number 30, the basis of the reckoning for each month. Thus was obtained for each hour a corrected sum of northerly and of westerly components, (southerly and easterly components being regarded as negative). These numbers are entered in the printed table, together with a column of *mean directions*, which are merely the angles whose tangents are found by dividing the westerly by the northerly component. These angles are reckoned from the N. point around through W. to S. and E., so that due west is deonted by 90° and due east by 270°.

The resolution into components, as just described, is effected very easily in a manner which may be illustrated by an example. Suppose that at a particular hour the wind has been observed to blow from the directions *a*, *b*, *c*, etc., the following numbers of times: 2, 1, 3, 0, 0, 5, 4, 7, 2, 2, 3, 1. To find the northerly component, these numbers are arranged in rows of three alternately forward

and backward, and the second and third rows receive the minus sign, as follows:

2	1	3
−5	−0	−0
−4	−7	−2
1	3	2
−6	−3	+3

The northerly component is, then, — 6 sin. 75° — 3 sin. 45° +3 sin. 15°.

But since sin. 75° = sin. 45° + sin. 15°, the above reduces to — 9 sin. 45° — 3 sin. 15°; whence, if the multiples of these two sines are arranged in a table, the value of the component may be written at once.

As will be seen from the foregoing account of the method of preparing the table, any number in the column of mean directions is derived from the corresponding number in the columns of components, and depends only on their relative, not their absolute values. The mean directions are therefore much less accurate an index of the changes of wind-direction than the components; and on this account it has not only been deemed essential to insert the columns of components, but it is on these columns chiefly that the following remarks in relation to the law of daily change will be based. Were a reduction of several years' observations, rather than the present exhibit of those of nine months, to be made, probably the best way of computing the true law of diurnal change in its exactness would be to first represent, by a series in the form of Bessel's periodic function, the law of change of each component, and from these compute the hourly values of the mean direction. The present table can do little else than indicate the existence of such a law, with the character of its most salient features, while any attempt to deduce exact results (*e. g.*, to obtain accurately the relation in time of the maximum of temperature to that of southerly wind), must be left to those who can deal with longer series of observations. It is believed, however, that this present table justifies the following as conclusive;—

The winds from the northern half of the sky attain their greatest prevalence (in point of frequency) over the southerly winds about the time of sunrise, and this prevalence is most diminished, or lost in that of the southerly winds, in the early part of the afternoon. This rule appears to hold in each of the months examined, without exception.

The easterly winds attain a decided prevalence over winds from the west, about, or somewhat after the middle of the day; and this, in each of the months examined excepting March, is the time of their maximum frequency.

This time of maximum frequency of east winds exhibits a tendency to occur earlier in the warm months than in the cold ones, and the same appears to be true of the time of greatest frequency of south winds.

The south and east winds prevail more decidedly over the north and west, not only in the warmest hours of the day, but, taking the whole day together, in the warmest months of the year.

The range of northerly components, or the difference between the values of this component for the two hours in which it reaches its highest and its lowest point, is as follows for each of the months examined: Nov., 38.8; Dec., 16.5; Jan., 21.2; Feb., 17.4; March, 25.5; April, 13.2; May, 24.3; June, 34.5; July, 38.8. For the westerly component the corresponding ranges are: Nov. 19.8; Dec., 18.7; Jan., 18.4; Feb., 15.1; March, 9.8; April, 7.6; May, 8.8; June, 14.2; July, 13.0. Each of these series shows a pretty well-marked minimum, which in the first case appears to fall in late winter, in the second case in spring. This is the more remarkable since in some parts of the country, unless I am mistaken, the month of March is that in which a diurnal change in wind-direction makes itself most apparent.

The usual order of daily change in direction at this place appears to be as follows: Beginning at sunrise from a point not far from due north, the wind *veers* until about the middle of the afternoon, when it blows from the southeast, and thence *backs* until the following sunrise.

During that part of the day in which the easterly component usually prevails over the western, there occur in the three months, February, March and June, 1884, isolated hours in which the opposite relation holds. A reference to the column of mean directions suggests an explanation of the anomaly by indicating that the wind has proceeded from southeast to southwest, and so is merely veering to a greater extent in these months than in others. The correctness of this explanation depends on the question whether the westerly winds, which have produced the effect noticed, were really from the southwest, or whether they were winds from points somewhat north of west, whose northerly components were neutralized by the southerly components of the (normal) southeast winds, while their

greater westerly components prevailed over the easterly factor of their antagonists. The table itself does not decide this question, which can only be answered by reference to the records. These show that the fact was as last stated, so that the anomaly is real and the explanation illusory. The excess of the westerly component here is due to the temporary prevalence of a wind quite characteristic of this region, and known locally (and somewhat facetiously) by the name of "zephyr." In the winter and spring it frequently blows with considerable force and fury, usually from a point slightly north of west, and is characterized by marked warmth and dryness—qualities which suggest an origin similar to that of the Swiss Föhn. The wind of the upper air (it may here be noted) is nearly always from the west, as indicated by the movement of the highest clouds.

If the table, in the instance just considered, is liable to suggest an error by representing the mean of west and southeast winds as southwesterly, it should be noticed that this fault is not to be laid to the charge of the special method of reduction employed. The same effect would have followed if, for instance, the numbers expressing in degrees the directions of the winds of the several days of the month had been arithmetically averaged. Again, every mode of reduction must leave it uncertain whether the usual change of direction really takes place as gradually as the table appears to indicate, or more suddenly. For if the wind changed suddenly every day from north to southeast, but at different times on different days, so that the change sometimes happened before ten, often before twelve, usually before two, and always by three, it is obvious that in the mean of directions for many days there would be a gradual change from north to southeast, occupying the hours from ten to three. The fact at Colorado Springs is that the change in direction is frequently quite rapid.

That the wind should veer during the day and back at night is no local peculiarity, for such is described as the normal law of diurnal change. On a uniform land-surface in north latitude, a person who would point out at each hour of the day the direction of the greatest neighboring masses of rarefied air, must point somewhat south of east in the morning, east of south at noon (since the greatest heat is east of the region where the sun is on the meridian,) and south of west at night. But the wind set in motion by the expansion of these masses will, according to the law of cyclonic

action, not move directly toward them. Instead of blowing from a point somewhat north of west at sun-rise, it may be deflected so as to come from due north, and a like amount of deflection will cause it to blow at noon from a point in the northeast quarter, and from a southeasterly direction at sunset. In the summer, when the range of the sun's azimuth is greatest, the diurnal change of wind-direction may be expected to exhibit a corresponding maximum of range.

But Colorado springs is by no means such a station as above imagined, in the midst of a uniform surface of land. On the contrary, the contrast is most marked between the great plains which stretch unbroken to the east and south, and the lofty mountains which fill the region to the west. A hundred degrees of the horizon are occupied by mountains. Pike's Peak, the highest among them, rises eight thousand feet above our level at a distance of twelve miles nearly due west. About as far north is the "Divide,"—a long, rounded water-shed, separating the basins of the Arkansas and Platte. It is about 1500 feet above the level of the town (or 7500 above the sea), at the nearest point, and extends eastwardly from the mountains, forming a prominent feature of the landscape for fifty miles.

The eastern slope of the mountain range, turned toward the sun in the forenoon, probably produces a region of rarefied air to the west and southwest of this place, at an earlier hour than would otherwise happen, thus making the transition from a north to a southeast wind both earlier in time and more sudden in character than would be the case in the ideally-situated station above mentioned: while as this slope is averted from the sun during the late afternoon, the progressive change in the wind's direction is broken off several hours before sunset, and the backing of the wind begins. In these suggestions toward an explanation of the observed phenomena,— put forward with much diffidence in view of the tenuity of the material on which they are based,—there is implied a very qualified analogy to the land and sea breezes of coast stations. Whether the true analogy is more close must be left to the judgment of the reader.

F. A. Loud.

THE THUNDER-SQUALLS OF JULY 5TH.

II.—THEORETICAL CONSIDERATIONS.

The first question which arises in the theory of the squall is its relation to the extensive system of cyclonic winds which pass over the country.

It will be seen by a comparison of Maps I and II in the November issue that all of the squalls of July 5th occurred in the southern quadrant of the cyclonic system. The two German squalls, and three destructive squalls—besides those above mentioned—which occurred in the United States during the summer of 1884 were all in the southern quadrant of cyclonic systems. The southern quadrant—and probably its extreme limit where the pressure is nearly normal—would thus appear to be the squall region, just as the southeast quadrant has been found to be the tornado region.

The southern quadrant lies between the area of greatest abnormal heat and the area of greatest abnormal cold accompanying cyclonic systems; and "Köppen suggests that squalls result from a too rapid *lateral* change of temperature, coming from the close approach of cool westerly to warm southerly winds, with an approximate vertical plane of separation; the squall is, then, the sliding of the cool air under the warm air, giving somewhat the appearance of rotation on a horizontal axis, while the tornado has a well pronounced rotation about a vertical axis; and these rotations are in opposite directions."—This JOURNAL, p. 159.

Köppen further supposes that the squall's action is confined to the air within less than 800 meters (875 yards) of the earth's surface. I do not think, however, this idea is sustained by facts. Neither in the squall of July 5,* nor in the German squall of Aug. 9, 1881 as Möller has shown, could cool air from near the earth's surface have entered the squall from the rear; since in neither case was there any considerable air movement in the rear, and the squalls moved away from this air at the rate of thirty or more miles an hour. Neither could this cool air have descended from the rear of the squalls; for, since air heats and increases its capacity for moisture on descending, there could scarcely exist a cloud, much less precipitation, in a descending current of air; yet it rained at the various stations from one to three hours after the passage of the squall of July 5. It is conceivable that between the base of the clouds and the earth's surface a narrow wedge of air moving with immense velocity might have come in from the rear, and have produced the phenomenon in question; but it seems so improbable on the face of it that it is useless to discuss it. Möller in discussing the German squall of Aug. 9, 1881, recognizes the impossibility of the cooler air having come in from the rear, and supposes that the whirl around a horizontal axis was only a part of the motion in the squall, but that the air revolving around this axis was the same air continuously whirling over and over again. His hypothesis, as I understand it, is that the heated air ascends according to the same law that air ascends in ordinary cumulus clouds. After precipitation begins, the air under the cloud becomes cooled by the falling rain and hail, and increases in density

* When "the squall of July 5" is mentioned in this article the great afternoon and evening squall of that date is referred to.

and pressure over the air in front of the cloud. The pressure gradient thus formed causes the air to rush out toward the front of the cloud; but, after it rushes out, it soon looses its velocity by friction with the earth's surface, and is finally lifted up by new air rushing out from under the advancing cloud. The air thus lifted up again comes under the squall cloud, is farther cooled, sinks, and again rushes out, going again and again through the same set of motions.

Möller thus supposes a long stretch of wind revolving around a horizontal axis, and moving over the earth's surface at a great velocity. The air in front is supposed to be continuously elevated—but he apparently forgets that there must be a supply of air from some source in the rear of the moving whirl.

If this be supposed to come in from the rear or sides, exactly the same difficulties present themselves as in the case of Köppen's hypothesis. Nor can it be supposed that the same air which ascends in front of the moving whirl descends again in the rear; for, since air heats up as much on descending as it cools on ascending, the air in the rear of the whirl would be as warm as the air in front, provided no precipitation took place; but, if precipitation has occurred, it would be as much hotter after descending, than it was before ascending, as would be necessary to revaporize all the water that fell. The air in all the squalls examined has, however, been found cooler in the rear than in front. Möller's hypothesis thus also seems untenable.

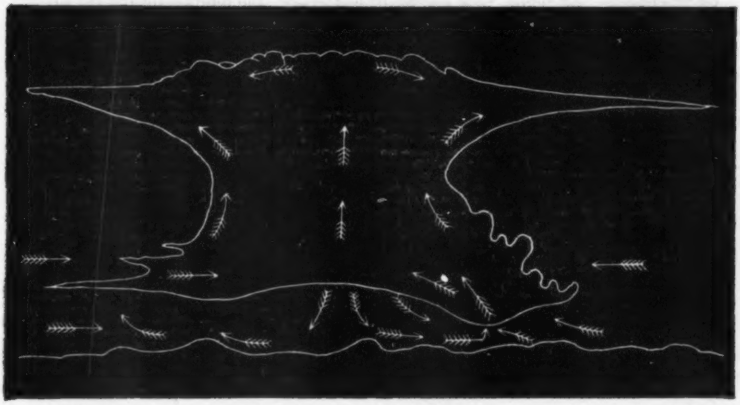
That there exists an ascending current of air within or above squalls is undoubted. In no other known way can the immense cloud and heavy rainfall be explained. The facts gathered concerning Köppen's squall, as well as the squall of July 5, furnish strong evidence that there was an ascent of air directly over the violent squall winds at the earth's surface. In the squall of July 5, these winds rushed with great velocity directly against a comparatively light southwest current. This could not have continued very long, without an immense compression and piling up of the air in front of the gale, unless this air had been drawn off somewhere, or elevated above the gale.*

At 6 P. M., when the squall was just north of Memphis, stations in West Tennessee reported a strong gale blowing from the north, while Memphis reported the highest wind velocity for three days from the south-west, from which direction it continued until about midnight. There was undoubtedly, then, a point somewhere north of Memphis where these two winds met and ascended. At 6 P. M., the wind was from the south-east at Cairo, while stations further west reported it from the north-west. Again they must have met and ascended. The reports from Louisville also gave evidence of an indraught toward the squall on its northern side. To explain the observed motions of the air at the earth's surface within the squall, however, it is necessary to suppose that there was a descending current right under the center of the squall cloud.

The accompanying diagram is intended to give an idea of what I conceive to be the shape of the squall cloud, and the motions of the air accompanying the squall, as indicated by observation. The dark portion at the base of the diagram is intended to represent the earth's surface, while the shaded portion above is intended to represent a cross section through the squall cloud, and the arrows to

* Several times during the last four years has the writer, about an hour before the arrival of an extended but slow moving thunder-storm, seen the wind shift from some southerly to a northerly or easterly quarter and blow directly toward the storm, but again shift on the arrival of the storm and blow strongly in the direction of the storm movement.

show the motion of the air in the squall. In the diagram, however, the vertical height is much exaggerated in proportion to the horizontal extension of the cloud.



The cloud accompanying the squall of July 5 must have been at least two hundred miles in breadth, and three hundred in length There is only one observation at hand from which to estimate the height. This observation was made by the writer at Estill Springs, Tenn. At that place the sky was, at 4.30 P. M., almost entirely clear except along the western horizon, above which rose a thick cloud-stratum to a height, as estimated by the eye, of 30°. The edge of this passed over head about 5 P. M., moving with great apparent velocity. This cloud-stratum was precisely like that which I have time and again seen projecting from the top of thunder-storms, and was, no doubt, the projecting top of the squall. cloud, since it continued to increase in density until the arrival of the squall. The squall at this time was travelling about fifty miles an hour. Supposing that the cloud top travelled as fast, that it rose 30° above my horizon at 4.30 P. M. and crossed the zenith at 5 P. M., a trigonometrical calculation shows that it must have have been 14½ miles high. This great height hardly seems probable, and making every allowance for error, the lowest possible height of the cloud I estimate as six miles, unless it is denied that this cloud-stratum was a part of the squall-cloud and moved with its velocity.

This is not the first time observation has seemed to indicate such a height to storm clouds. In the Chief Signal Officer's Report, 1873, p. 1081-2, Jas. Macintosh estimates from a number of observations that the height of the tornado cloud of May 22, 1873, must have been sixteen miles (or more). I have read similar estimates as to the height of certain storm clouds by others, but have'nt the references at hand. Thunder storms usually travel about 20 miles an hour, and observations with reference to determining their height would be interesting.

That an ascending current exists in small thunder clouds above an out-flowing (hence necessarily descending) current below has been undoubtedly proven by balloon observations and observations on smaller clouds in the vicinity of thunder clouds, and I am unable, with the observations in my possession, to see any distinction between ordinary thunder storms and squalls, excepting magnitude.

The view I have taken of the squall supposes the descending current under the cloud to be a small part of the ascending air, which coming near the base of the cloud is by some force again drawn down.

Its cooling is, no doubt due to the contact with the rain-drops, which unlike air do not necessarily,—except by contact with heated particles of air,—heat up on descending, but retain, to a greater or less extent, the temperature of the place where condensation takes place.

This must be a place of low temperature, since in all the squalls examined, in some parts of them, the precipitation was in the form of hail. Millions of such drops coming in contact with the air under the base of the cloud must necessarily cool it to a great extent. Instead, however, of the descending current under the cloud being the cause of the ascending current as Köppen has supposed, it is probably a detriment to it. The ascent of air in the squall-cloud is most probably the result of an unstable condition of the atmosphere, which arises whenever the rate of decrease of temperature is more than one degree (F.) in 183 feet of ascent. In such a case the air near the earth's surface is relatively lighter than the air above it; and, when an ascending current is pretty well established, the air near the earth's surface—which has become highly heated,—is pressed toward this vent, while the cooler air above slowly subsides.

As before shown, there was strong evidence that there existed an indraught toward the squall of July 5; and this necessitates the supposition, that within the base of the cloud the pressure was lower than at any of the corresponding altitudes in its vicinity. But a large number of observations with self-recording barometers in different parts of the world have shown, that at the earth's surface under thunder-storms and squalls there is an increased pressure.* This it is which causes the out-flowing air from under the storm. The velocity of the wind is even capable of being determined in terms of the pressure gradient thus arising, as shown by Köppen.

In slow moving or stationary thunder storms it has been found that the air blows out from the center with approximately equal velocities in all directions. In a paper read before the American Association for the Adv. of Science, 1873. Mr. Hiram A. Cutting described four violent, stationary thunder-storms investigated by him.

At the beginning of these storms a breeze was noticed at the earth's surface setting right toward the cloud; but immediately the rainfall, electrical phenomena, etc., had become pretty well established the wind began to blow out from the storm in all directions; and in a short time with such hurricane violence that crops and trees were leveled to the ground, and in several cases roofs of houses torn off. In every case the fall of rain and hail was immense, and the flashes of lightning and peals of thunder incessant. After about half an hour the storm would begin to abate, and rapidly disappear over the very spot where it was formed, leaving only a trace of cirrus to indicate its former existence. An examination of the fallen objects after the storm showed in every instance that in the center they were thrown indifferently in every direction, but on every side at a little distance from the center and out to the extreme limit of destruction the fallen trees and other objects pointed directly from the center. An exactly similar storm is described in the *American Journal of Science*, ser. 3, vol. VI, pp. 32–6, by Dr. Geo. Sutton.

When, however, a phenomenon of this kind has a rapid forward motion, as was the case in the squall of July 5, the motions of the winds as regards the earth's surface will be necessarily changed. The wind's velocity will be redoubled in front, decreased behind, and its motion deflected at the sides.†

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* See this JOURNAL p. 156 and Nos. 2 and 3 of *Das Wetter* for examples.

† The motions of squalls and thunder-storms is probably the result of the rapid motion of the upper currents into which they penetrate.

The phenomena connected with the stationary thunder-storms referred to show the inadequacy of a theory proposed by Dr. Cirro Ferrari, in which it is supposed that the wind accompanying and blowing out from under thunder-storms is the result of an indraught toward a depression immediately in front of the thunder-storm, and its violence due to the motion of the thunder-storm and accompanying depression.

In the endeavor to explain this increased pressure found under thunder-storms and squalls, two hypotheses have been advanced. One is that the falling rain-drops produce a compression of the air, and hence increase the pressure under the cloud.* The other is that the cooling of the air by the falling drops causes it to contract, and hence increase in pressure.†

One, or both of these hypotheses combined, have been thought by writers on the subject sufficient explanation of the phenomenon. The not unusual occurrence of torrential rainfalls without wind, and of wind accompanying clouds without rain seem, however, at variance with both.

In the *American Journal of Science*, ser. 1, vol. XXXIX, pp. 59–61, is a description by Mr. Wm. Gaylord of a thunder-storm in many respects very similar to the small stationary thunder-storms referred to before. This storm, however, had a slight motion toward the northwest. The observer states that, as it passed over him, "not the slightest wind in any direction could be felt, but the water poured down like a cataract." Hail and snow fell from the northeast edge of the cloud. "On the west side the thermometer was but little affected, but on the east and northeast side, the cold was perceptible, and the thermometer fell rapidly; but in neither case was there any apparent movement of the atmosphere to account for such a change." The cloud was also characterized by a small amount of electrical manifestations.

The facts connected with the squall of July 5 also seem at variance with both these hypotheses. The heaviest rainfall in the squall was between 5 and 7 P. M., while the heaviest wind as judged by the amount of destruction done was between 7 and 8 P. M.; and the wind continued for an hour or more after the rainfall had almost entirely ceased. The temperature-difference at the earth's surface between the air within and the air without the squall was greatest between 4 and 7 P. M.

Believing that some other cause must be sought in explanation of the increased pressure and outflowing winds under thunder-clouds, I have supposed it the result of electrical attraction, arising out of the immense difference of potential between the cloud and air under the cloud, and the earth's surface. The air under the cloud is probably not only cooled by contract with the drops falling from the cloud, but also raised to a very high potential.

In the cloud itself where precipitation is taking place, the upward buoyancy of the air must be very great. But under the cloud, where the air is near the earth's surface and cooler and denser than surrounding air, it does not seem impossible that electrical attraction might not only overcome what would otherwise be an upward tendency of the lower air due to a deficiency of pressure above but might produce a compression of this air which would give rise to an increased pressure such as is observed under thunder-clouds.

After precipitation has ceased, it does not seem impossible that electrical attraction might gain the ascendency over buoyancy, and the whole cloud be drawn downward, producing a strong wind at the earth's surface, and a rapid wasting of the cloud, such as is sometimes seen. H. H. CLAYTON, JR.

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* See Pat. Office Report, 1858. Art. by Prof. Henry, p. 494.

† See Espy's Fourth Met. Report, p. 106.

SOLAR HEAT AND TERRESTRIAL DILATABILITY.

By Sr. F. D. Covarrubias.

IV.—Regular Diurnal Oscillations of Atmospheric Pressure.

In this section we will occupy ourselves with a phenomenon which is general for the whole earth, but of which the intensity changes greatly with the latitude.

The periodic and regular variations of pressure have been explained up to the present time by the action of solar heat upon the atmosphere.

This doctrine has to support it the remarkable fact that generally the temperature of the air is greater when the barometric column is the smallest, and *vice versa.* In effect, it is toward three o'clock in the afternoon that the thermometer indicates the maximum of the temperature of the air, and it is also toward the same moment, or a little later, that the barometer signals the minimum of atmospheric pressure. A few hours before and after that of these extreme indications the two instruments have an inverse progression, that is to say, that one rises when the other falls.

In order to appreciate better the relative progress of the temperature and the pressure, we will make use of the thermometric and barometric observations made at the Central Meteorological Observatory of Mexico, where for four years indications from all meteorological instruments have been registered from hour to hour, day and night.

In the following table the hourly means are recorded, which we owe to the kindness of Sr. Mariano Barcena, director of the observatory, and which relate to the three years comprised between Jan. 1, 1878, and Dec. 31, 1880. The annual means are 15°.7 C and 586^{mm}.75.

Hours.	Temperature.	Pressure.	Hours.	Temperature.	Pressure.
	°	mm		°	mm
Mid-night.	13.3	587.18	Mid-day.	19.5	586.88
1	12.8	586.96	1	20.6	586.27
2	12.3	586.78	2	21.3	585.65
3	11.9	586.66	3	21.4	585.28
4	11.5	586.70	4	20.9	585.19
5	11.2	586.90	5	19.8	585.40
6	11.1	587.21	6	18.3	585.77
7	11.9	587.54	7	17.0	586.19
8	13.4	587.81	8	16.0	586.69
9	15.0	587.93	9	15.2	587.10
10	16.9	587.80	10	14.5	587.33
11	18.2	587.43	11	13.9	587.30
Mid-day.	19.5	586.88	Mid-night.	13.3	587.18

Our fig. 6 graphically represents the numbers of this table. The continuous line is the curve of the temperatures, and the line of points the curve of pressures,

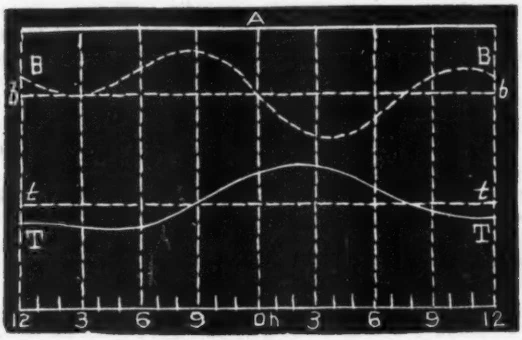

FIG. 6.

both having the hours for abscissas, and respectively the heights of the thermometric and barometric columns for ordinates. In the first the variation of each degree centigrade is represented by a millimetre; and in the second the variation of a millimetre of pressure has been made five times as great, so as to render the oscillations of the barometric column more perceptible. The straight lines *tt* and *bb* indicate respectively the mean temperature and pressure.

Except as to the difference of amplitude of the oscillations, the general figure and the principal sinuosities of these curves are almost the same for all parts of the earth; and, as we have said, it may be seen that the coincidence of the maximum of temperature and of the minimum of the pressure of the air, toward three or four o'clock in the evening, is almost perfect. But it should be remarked at the same time that the divergence of the two curves really takes place only during the presence of the sun above the horizon; and that during the night they retain a sort of parallelism, their minima at dawn occur at almost the same hour, and they rise together from that moment to the barometric maximum at nine o'clock in the morning. From this last hour the curve of pressure begins to descend, while that of the temperature continues to ascend. It will be understood, then, that the foundation of the explanation that has been given of the barometric oscillations, that is to say, the inverse progress of the temperatures and of the pressures of the air, is not strictly true, except toward the extreme indications in the afternoon, or at the most for the periods of mid-day and evening; that it is less satisfactory for the barometric maximum of nine o'clock in the morning, which coincides with no thermometric minimum; and that it is not at all satisfactory during the night and a part of the morning.

But, besides, if the diurnal oscillation of the barometer is solely produced by the action of heat upon the atmosphere, how does it happen that it manifests itself with such regularity and intensity in the intertropical zone, that is to say, there where the temperature of the air is so uniform? And, on the contrary, how is it that in the countries the farthest removed from the equator, where the alternations of heat and cold are so great, it is so small?

In the northern regions the commotions of the atmosphere give rise to great perturbation in the indications of the barometer, by means of which we have even been able to predict the future state of the weather; while in the equatorial regions

tempests, hurricanes, and other perturbations of the atmospheric equilibrium, remain almost unperceived by the barometer, which, even in the midst of these circumstances, signals none the less the periodic oscillations, almost without important alteration. How, then, would it be possible to accept the solar action upon the atmosphere as the only cause of such contrary phenomena, and, above all, in the midst of conditions in which we expect precisely opposite results from those observed? The successive heatings and coolings of a column of air are not of themselves sufficient to explain the variations of its weight, since the expansions and contractions, which are the immediate consequence, do not change the quantity of matter which it contains.

We must, then, have recourse to the formation of currents provoked by the differences of temperature, which are, besides, in perfect accord with the laws observed in heating gases, and with the molecular mobility which characterizes them; but has anyone proved anywhere the existence of these currents? In the vicinity of the equator they should be very perceptible.

We understand well that some of the objections which we raised against the theory of the action of heat upon the atmosphere, to explain the regular oscillations of its pressure, would be equally applicable to ours, which is that of solar action upon the earth; but there are others from which it is free, in our opinion, and even those which can be applied to it have really no force except as it is admitted that the laws of heating of the air are the same as those of the heating of the soil, notwithstanding the enormous difference of their diatherminous properties; and notwithstanding the foundations of our opinion with regard to the different ways in which heat acts as the real cause of the warmth, and as cause of dilatability. We have certainly not the right to deny to the atmosphere the participation which it might have in the production of the phenomenon of the regular oscillations of the barometer; we believe, on the contrary, that the facts as they are observed come perhaps from the double influence of heat upon the air and upon the earth; but our views with regard to its intermittent action as an agent of dilatation lead us to admit that the latter is the greater. We are going, then, to try to explain these oscillations by the effect alone of the successive dilatations and contractions of the surface of the earth. In rising this would naturally cause a diminution of atmospheric pressure, since the column of air which gravitates above it would become, thus, really less, by a quantity equal to that elevation, and, in contracting, it would receive the additional weight of the column of air of which the height will be equal to that of the shrinkage of the earth. The elimination of atmospheric action does not signify, we repeat it, the negation of its influence, which does not coincide, perhaps, with that of the earth, as to maxima and minima effects; but we may regard them as arising from two causes, each of which would act with the intensity natural to it; and the final effect as the result of a single differential cause of which the intensity would be equal to the difference of intensity of the causes.

In our first section we have found that the dilatation at the equator being taken for a unit, that of a place of which the latitude is l, would be given by the equation: $\frac{dr}{da}=\frac{\cos.^{2}l}{s}$.

Let us look for the place, which according to our theory should exist between the elevations and depressions of the soil, produced by its dilatability, and the corresponding oscillations of the barometer. For this purpose reflect that the first are only the differences of level measurable with the barometer; and that, in consequence, we could employ any formula of barometric leveling. However,

as in the actual case the maximum difference of level $2dr$ should be always small, we will employ the most simple formula: $n=3995\,D\frac{(B+b)(B-b)}{Bb}$, which we have developed in our *Traité de Topographie, Géodésie et Astronomie* (vol. 1, page 520), and which is applicable up to about 1000 meters of difference of level. In this formula n represents the difference of level, or $2dr$ in our case; B the height of the barometric column at the lower station; b the same indication at the upper station; and D the coefficient which depends on the mean temperature of the air between the two stations. This temperature being o, we have $D=1+0.004o$, so that its value can be taken as equal to 1, when it is a question of only small differences of level. However, for greater precision, and to simplify the calculation, we will adopt $dr=2000\frac{(B+b)(B-b)}{Bb}$.

If, now, we designate by o the regulur oscillation of the barometer in a part of the globe of which the terrestrial radius is r, and by B the mean height of the barometer, we will see that, in the moments of the greatest contraction and dilatation of the soil, the barometer will indicate respectively $B=B+\frac{1}{2}o$, $bB=-\frac{1}{2}o$, from which may be deduced: $B+b=2B$, $B-b=o$, $Bb=B^2-\frac{1}{4}o^2$. The barometric oscillation being, even at the equation, only a few millimeters, the term $\frac{1}{4}o^2$ is always very small, and may be neglected without appreciable error. Our formula will become then, by the substitution of these values, $dr=4000\frac{o}{B}$.

And this equation will establish the relation which we seek between the oscillations of the barometer and of the soil.*

Designate, now, by Δ the barometric oscillation at the equator at the level of the sea, and the mean height of the barometer by A. The formula will be in this case: $da=4000\frac{\Delta}{A}$, and its relation with the first: $\frac{dr}{da}=\frac{Ao}{B\Delta}$. But we have found $\frac{dr}{da}=\frac{\cos.^2 l}{e}$, and, in consequence, we will have finally: $o=\Delta\frac{B\cos.^2 l}{Ae}$, an equation which will serve to calculate the barometric oscillation for a place of latitude l, knowing that which corresponds to the equator or *vice versa*. It establishes, at the same time, the law of the variation of this phenomenon from the equator to the poles, if, according to our theory, it is produced by the successive contractions and dilatations of the surface of the earth.

Following the plan that we have traced for ourselves, let us compare the result of the theory with the facts observed; for this purpose, we will make use of the following numbers which we borrow from the *Traité de Physique* of M. Ponillet,† the only complete ones that we know, and which show the regular oscillation of the barometer corresponding to the mid-day period, observed from the vicinity of the equator to beyond the polar circle. We have arranged them in the order of increase in latitude, in adding to them the observations in Mexico and those which we made in Guatemala.

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* The formula of M. Babinet, $n=16000\,D\frac{B-b}{B+b}$, will give the same results. Making, in effect, $D=1$, and putting in the values of $B-b$ and of $B+b$, we find immediately: $n=2dr=8000\frac{o}{B}$

† Mexican translation, vol. II, page 280.

Places.	Latitudes.	Altitudes.	Oscillations.
Quito	00.00	2908^m	2.52 mm
Boyrta	4.35 N	2660	2.39
Paita	5.00 S	00	3.40
Equatorial America	from 23° N. to 12° S.	0^m to 3000^m	2.55
Guaira	10.36 N	00	2.44
Guatemala	14.13 N	1500	2.27
Mexico	19.26 N	2260	2.74
Rio Janeiro	22.54 S	....	2.34
Las Palmas	28.00 N	....	1.10
Cairo	30.03 N	....	1.75
Marseilles	43.10 N	....	0.72
Toulouse	43.34 N	....	1.20
Chamberg	45.34 N	274	1.00
Clermont-Ferrand	45.46 N	410	0.94
Strasbourg	48.34 N	....	0.80
Paris	48.50 N	....	0.78
La Chapelle	49.55 N	....	0.36
Koenigsberg	54.42 N	....	0.20
....	74.00 N	....	0.00

Unfortunately the fourth of these numbers, which would be so interesting to calculate the equatorial oscillation, embraces very extended limits in latitude and in altitude above the sea. In order to be able to utilize it, we are, then, obliged to adopt 5° 30′ for mean latitude and 1500^m of altitude, or 631^{mm} of mean barometric pressure. Among the others there are some which are not accompanied by the altitude at which these observations were made, or by the corresponding height of the barometer.

Our formula giving $\Delta = o\frac{Ae}{B \cos.^2 l}$ will serve us in calculating the value of the equatorial oscillation by means of all the observations made between the tropics, where it varies very little with the altitude. Taking $A = 758^{mm}$, which is the mean pressure at the equator, and the values of B and l corresponding to each place of observation, we find the following results:

Places.	l	B	Δ
Quito	00.00	527 mm	4.05 mm
Bogota	4.35	544	3.36
Paita	5.00	760	3.43
Equatorial America	5.30	631	3.11
Le Guaira	10.36	760	2.56
Guatemala	14 28	640	3.74
Mexico	19.26	587	4.47
Rio Janeiro	22.54	760	2.99
		Mean	Δ = 3.46 mm

With this mean value of $\underline{\Delta}$, upon which depend those of o for all the latitudes, we can now calculate the mid-day oscillations for all the places where they have been observed, in order to compare the results of the calculation of our formula with those of direct observation, and see, thus, to what point the theory is capable of representing the facts.

We will have then: $o = \underline{\Delta}\frac{B\ \cos.^{3}l}{\Delta e}$, and for every place we will employ the value of B which corresponds to its altitude above the level of the sea. In cases where the last element is not given in M. Ponillet's table, we have adopted a mean pressure of 761^{mm} for the places situated at the level of the sea, which is that which corresponds to the latitudes from 40° to 60°. For Paris, Strasbourg, Chamberg, we have taken B=756^{mm}.

The following table shows the results of the calculation beside those of observation and in the last column the differences $o—c$ between the two:

Places.	B	Oscillations.		$o—c$
		Observed.	Calculated.	
	mm	mm	mm	mm
Quito	527	2.82	2.40	+0.42
Bogota	544	2.39	2.43	−0.04
Paita	760	3.40	3.38	+0.02
Equatorial America	631	2.55	2.51	−0.26
La Guaira	760	2.44	3.74	−0.80
Guatemala	640	2.27	2.54	−0.27
Mexico	587	2.74	2.12	+0.62
Rio Janeiro	760	2.34	2.46	−0.12
Las Palmas	760	1.10	2.11	−1.01
Cairo	761	1.75	1.94	−0.19
Marseilles	761	0.72	0.96	−0.24
Toulouse	756	1.20	0.94	+0.26
Chamberg	734	1.00	0.81	+0.19
Clermont-Ferrand	720	0.94	0.70	+0.16
Strasbourg	756	0.80	0.67	+0.13
Paris	756	0.76	0 66	+0.10
La Chapelle	756	0.36	0.59	−0.23
Koenigsberg	756	0.20	0.38	−0.18
....	760	0.00	0.02	−0.02

The general smallness of the differences $o—c$ and the diversity of their signs show that the observation and the calculation agree among themselves as much as it was possible to hope. The mean +0.24 of the positive differences and that of −0.31 of the negative differences give in the whole of the observations a mean discordance less than −0.08. We believe, then, that we should not expect a more perfect accord between the theoretic and abstract expression of a phenomenon and its real accomplishment more or less influenced by local irregularities or perturbations.

These influences, in our opinion, present themselves especially in the interior of continents, and in general, in a more striking manner, in the countries which are much elevated above the level of the sea. Thus, for instance, the largest values of equatorial oscillation Δ have been furnished by the observations from Quito and Mexico, while those of La Guaira and of Rio Janeiro have given the smallest ones. Would a differential effect make itself felt, in the vertical sense upon a mass elevated above the ground, in such a way as to render the dilatation greater than it would be at the level of the sea in the same latitude?

Could more or less abundant vegetation produce an analogous modification of the general phenomenon? We do not know the conditions in that respect of the city of Quito; but as to those of Mexico we can say that that capital is situated almost in the center of the high plateau of Anahuac, which is characterized by a scarcity of vegetation, poor in foliage and composed principally of different species of cactus. At this height the air usually contains but little moisture, and the very dry soil of these immense plains is exposed almost without defense to the rays of the sun, of which the heat should produce, in consequence, more noticeable effects than would be produced in other circumstances. The study of these diverse influences upon the phenomenon of the regular oscillations of the barometer would offer, then, in Mexico a special interest; and we venture to engage the physicists and meteorologists of our country to undertake it, as proper to throw light upon the modifying causes which should influence it. Between the capital and the Gulf, in a distance of from 300 to 400 kilometres, almost upon the same parallel of latitude they could make their comparative observations, in taking advantage of the great altitude of the Volcano Popocatapetl, which surpasses 5000 metres.

We know, besides, that the influence of the continents upon the movements of the barometer has been generally recognized, and that it seems to always be more regular in the islands and at the level of the sea. Our theory should depend upon this fact, since the homogeneity of the water should make its dilatation more regular than that of the earth, the corresponding oscillations of the barometer would be in the ocean, and in equality with all the other circumstances, less subject to local perturbations than in the interior of the continent. It should, however, be well understood that the regularity of which we speak is only relative to the homogeneity, and to the greatest independence of external causes; but that the dilatability of water is itself irregular enough. In effect, from the recent experiments made at Washington by Mr. Hubbard, we will take the following numbers, which express the volumes of salt water of the ocean, submitted to different temperatures, the volume which it will have at 15°.6 being taken for the unit:

Temperatures.	Volumes.	Temperature.	Volumes.
4°.4	0.99823	48°.9	1.01218
15.6	1.00000	60.0	1.01804
26.7	1.00309	71.1	1.02460
37.8	1.00716	82.2	1.03192
48.9	1.01218	93.3	1.03993

By these observations we see that the co-efficient of cubic dilatation of the water of the sea has values increasing with the temperature and that in the limits

of the preceding numbers it varies from 0.00016 to 0.00065, its mean value being about 0.00047.

Since we have incidentally given our attention to the dilatation of water, we will not leave the subject without making a few reflections which seem to us worthy of attention, and suitable to support our ideas upon the successive rising and falling of the soil by the action of heat at different hours of the day and night. The observations which we have just cited indicate that the dilatation of the water of the seas is quite large, and on the other hand experiment teaches us that the solar heat penetrates into the ocean to a great depth.*

This heat acts in a very variable manner in the course of the day, since from the horizon to the zenith its intensity grows, following the relation $\frac{1}{e}\cos.l$, in which l represents the zenith distance of the sun. The law of variation is then, very rapid, and its effect very persistent about noon, that is, when its intensity is strongest; the dilatation of the water should experience, in consequence, considerable variations and the corresponding variations of its volume would make themselves felt, on the shores, by the elevations and depressions of the sea, if the banks themselves remained immobile, that is if the soil did not follow almost the same movement.

Water is probably more dilatable than the earth; but we believe that as to the final effect, there is a certain compensation, due to the fact that water being more diathermanous than the earth, heats and cools less than the earth, and, that its great mobility facilitates the formation of currents in which a part of its dilatation resolves itself. However, if we study attentively the tides from the point of view of the influence of heat, and especially the differences between the predictions and the facts as they take place, the manifestations of differential effect between the dilatations of the earth and water may be noticed.

Some changes of the level of the sea as compared to the coasts have been often noticed, and attributed to differences of atmospheric pressure.

In Kaemtz's meteorology we have found the following note. "Atmospheric pressure exercises an influence upon the mean level of the sea; this level, which is obtained by taking the mean of two consecutive full seas, and the height of the lower intermediate sea is generally regarded as constant. But M. Daussy, in comparing the observations at Brest among themselves, found that it was not constant, and that the mean level varied according to the barometric pressure." After this paragraph, the author gives a table of comparisons, which show, by its extreme numbers, that 20 millimetres of difference of pressure corresponds to 840 of difference of height of the sea.

These differential effects have been noticed, then, but we cannot admit the explanation given by them. According to this, a part of the atmospheric mass of which the upper limits are indefinite, notwithstanding its lightness, its complete mobility and the perfect elasticity of its particles, has been capable of producing the depression of a fluid which is neither elastic nor compressible, and which can never leave the limits which the coasts give. What then is the force, which, in the rest of the mass of air, can oppose itself to the lateral transmission of such an augmentation of the pressure of the atmospheric column of which the weight is sufficient to depress the Ocean? Instead of saying that the difference of pressure of the air modifies the height of the mean level of the sea, would it not seem more comprehensible to say that the differences of height of the sea, what-

*Observations made in the tropical oceans show that the temperature of water, being at the surface from 28° to 30°, is about 6° at a depth of 500 fathoms.

ever may be the cause producing them give rise to corresponding modifications in atmospheric pressure?

But to return to our principal subject of the regular oscillations of the barometer. If we had the right to suppose them produced entirely by the single effect of the successive dilatations and contractions of the earth's surface, or if we believed ourselves to have a sufficient title for the acquisition of such a right by the fact of the remarkable accord between the oscillations calculated by our theory and those furnished by observations, it would be easy for us to calculate the maximum of the swellings and depressions of the soil for any latitude. In effect, our preceding formulas give:

$$da = 4000 \frac{\Delta}{\Lambda}, \quad dr = \frac{\cos.^3 l}{e} da,$$

the first serving to calculate the maximum swelling at the equator, and the second that which corresponds to the latitude of l. Without affirming anything with regard to the real size of these movements of the soil, we will make the calculation, if only to acquire an idea of the values that they should have in order to produce of themselves, the phenomenon of barometric oscillations. The knowledge of these same magnitudes might also serve as a starting point for new researches, and for imagining new experiments capable of confirming or correcting our provisional results.

The following table shows the results of calculation:

l	dr	Diff.	l	dr	Diff.	l	dr	Diff.
	m			m			m	
00°	18.258	m	30°	10.288	m	60°	1.159	m
10	17.179	1.079	40	6.308	3.980	70	0.259	0.899
20	14.241	2.938	50	3.139	3.169	80	0.018	0.241
30	10.288	3.953	60	1.158	1.981	90	0.000	0.018

The above numbers, and especially those which relate to the equatorial region, might appear too great as expressions of a vertical movement of the soil—the more as they express only half the total amplitude of this movement. Nothing, in effect, seems to disprove them, and notwithstanding that from lack of terrestrial benchmarks and the habit which people have of considering everything which has any relation with the solid part of the globe as perfectly stable, the fact is, the mind has always refused to admit the cessation of that stability, without proofs which are very perceptible to our senses. Dead or inorganic matter has been long regarded as absolutely deprived of any kind of activity; and notwithstanding we know to-day that it is, on the contrary, in a state of perpetual activity, under the influence of chemical affinities and reactions, or under those of heat, of electricity, etc. And, on the other hand, is the vertical movement of the earth's surface, always very small compared to the immense extent where it is produced, more incomprehensible than would be the failure of a general property of matter, such as dilatability? Here is how we conceive of the oscillations of the soil in relation to the mean condition, whatever be their amplitudes, and however they may accord with the corresponding ones of the barometer. The substances composing the surface of the earth, endowed with a sufficient mobility, cede at the end of a certain time to the continual action of the heat of the sun, swell and elevate their surface as compared to the substances which are found at 90° of distance upon the limits of the illuminated hemisphere.

These are, in the mean time, compressed by the minimum of solar influence, in such a way, that the hemisphere which has the sun S (fig. 7) above its horizon at a given moment, presents a culminating point A, from which the surface descends gradually to the limiting circle B B′ of which the points are, on the contrary, depressed.

This depression will produce in its turn, by the mobility or the compressibility of terrestrial matter, a very small elevation toward the point diametrically opposed to the culminant A, and it results from it that we shall have simultaneously two elevated parts, although in a very different degree, and two others depressed below the mean surface of the earth, in the same manner that the tides are produced in two regions on opposite sides of the globe. The culminating point A follows the sun at a distance of about four hours; it is this which gives rise to barometric minima; and the most depressed points, distant about 98° or nearly 6 hours from the culminant, produce the maxima of atmospheric pressure.

We should avow that it is only by analogy that we try to explain thus the slight elevation opposite A, which responds to the minimum of day-break. In a fluid or liquid mass, which is sufficiently elastic, this effect of compression does not seem, to us, difficult to conceive; but it is for scholars to judge, if, in the case of terrestrial substances, it would be equally explicable by a simple reaction.

The oscillation of midnight is, besides, very small as compared to that of midday, and even that of the evening. The means of three years of hourly observation made at Mexico give, for all the oscillations and the duration of their respective periods, the following numbers:

Oscillations.	Amplitude.	Period.	Ratio.
Mid-day	mm 2.74	7 hours.	1.000
Evening	2.13	6 "	0.777
Mid-night	0.66	5 "	0.241
Morning	1.27	6 "	0.464

The fractions in the last column express the ratios of the oscillations, that of mid-day being taken as unity. If, as we think, their relations as to size are nearly the same for the whole earth, these fractions could serve to calculate the different oscillations (in want of direct measurements) in terms of that of mid-day.

From the above numbers we see the smallness of the midnight oscillations. They have been used in constructing our figure 7, although with the exaggeration necessary to make the elevations and depressions of the soil perceptible as compared to the circle of points which indicate its mean position. In order to render the exact proportions to the curve, it would be necessary to construct it upon a circle of more than 800 m. radius, the spaces between remaining of the same size as they are in the figure. It is thus that it represents really the form that the parallel of latitude of Mexico would take, according to our hypothesis; and it will be understood that the deformation becomes less and less perceptible in proportion as the latitude increases, because of the rapid diminution of the amplitudes of the barometric oscillations.

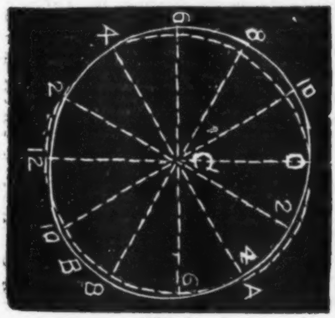

FIG. 7.

We end here our thermogeodetic work. There are certainly many other phenomena which could be attributed to it, and of which the attentive study, would relieve more than one doubt, and clear up more than one obscure point, but unfortunately the lack of time, sufficient data, and many other indispensable elements prevent us from undertaking it. It is especially we repeat it, to ask the co-operation of savants, that we have ventured to publish our reflections upon the influence that solar heat, seems to us to exercise upon the figures of the earth, as much by its constant action as by its intermittent action. They can, besides, count upon our personal activity, which we place at their disposition, and we venture to say also upon that of the enlightened youth of our country always enthusiastic for science. We will follow their counsel and their instructions to make any experiments which they will confide to us, for we believe that it is principally in the tropical zone that these works will give the most appreciable and decisive results.

In exchange we call their attention to the different phenomena mentioned in the course of this memoir, and in particular to the heating, dilating, and refrangible properties of the different rays of the spectrum of the sun, which have already shown very perceptible differences in their heatiug powers. These studies would be proper to confirm or combat our hypotheses upon the effects, not necessarily simultaneous or proportional, of heating and dilatation. As to the observation of other natural facts we could mention those of the tides in comparison to their predictions; those of marine currents, and of the singular property, which we believe we have noticed in them, to divide in order to approach the coasts, in leaving an interval perfectly calm, and almost always toward 30° of latitude, like the Saragassa Sea of the Atlantic Ocean. This last fact as well as that of the calm belt in the zone of calms, existing near the same region, seems to us to indicate that in the vicinity of the third of the quadrant, or near the commencement of the extra tropical zone, something exceptional and abnormal occurs in comparison with the general laws which rule the rest of the earth. It has seemed to us the more striking, that they accord better with the indications of our theory, which signalize a relative depression of the earth's surface, precisely where we know the existence of parts of the ocean and of the air which are comparatively calm, veritable lakes in the ocean, and in the atmosphere.

But it is above all from geodesy and astronomy that we must hope for the practically exact determination of the true figure of the earth. Unfortunately nearly all the works executed up to the present embrace only the mean latitudes; only two or three geodetic measurements have been made in the vicinity of the equator and of the polar circle, none that we know of near 30° or 35° of north latitude. This last would be, however, full of interest to verify the results obtained by *La Caille* in the southern hemisphere, and which our theory should have been able to forsee. We have also reported the special importance of the degree of the meridian measured at the latitude of 54°44′, as suitable to furnish the length of the equatorial radius independently of the value of the eccentricity.

Astronomy counts to-day upon the powerful auxiliary of the electric magnetic telegraph to determine the differences of longitude; so that the geodetic operations executed the length of the parellels are at present susceptible of the same precision as those of the arcs of the meridian. They probably constitute the most sure means of knowing whether the earth is or is not a solid of revolution, and even the means of furnishing in the first case previous data for the determination of the figure of its generater.

The general result of this paper can be summed up as follows:

1st. The whole of the geodetic measurements executed up to the present, the general means of barometric observations made at the level of the sea and those

of the regular oscillations of the barometer, seem to indicate that the action of solar heat is not inappreciable at the surface of the globe, as cause of dilatation.

2d. The same observations, and some other physical phenomena, seem to accord in indicating a depression of the terrestrial surface, toward the third of the quadrant of the meridian, in relation to the curvature which it should have if its generator was exactly elliptical.

3d. The regular diurnal oscillations of atmospheric pressure, and their decrease from the equator to the poles, are explicable by the variable action of the sun upon the soil; or, in other words, this phenomenon is accomplished as if the alternate heating and cooling of the soil produced little successive elevations and depressions of the surface of the earth, or a slight vertical oscillation.

4th. The theory, established abstractly, in harmony with the general properties of matter and appearing to be verified by the observation of the facts mentioned, indicates that the law of their diminution is that of the direct ratio of the cube of the cosine of the latitude and in inverse ratio to the oblique height of the atmosphere.

5th. It is to be desired that direct experiments should be made to try to measure the dilatation of the soil, and that in future geodetic works this same effect of heat may be taken into consideration.

6th. Besides the measurements of the arcs of meridians and of parallels in the vicinity of the equator, and in the highest latitudes, it would be well to measure these arcs between the latitudes of 30° to 35°, as a means of verification of previous work, or for the most exact determination of the figure of the earth.

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LITERARY NOTES.

JOURNALS.

Meteorologische Zeitschrift. Herausgegeben von der deutschen meteorologischen Gesellschaft, redigirt von Dr. W. Köppen, Hamburg, Seewarte. Berlin, Asher & Co. Erster Jahrgang, 1884; Heft 9. September.

(1) GROSSMANN: Die mittlere Bewölkung einer Periode als Funktion ihrer hellen und trüben tage, p. 341–348.

The author finds that the mean percentage of cloudiness (B) of a series of stations for a period of n days is represented by the empiric formula $B=53+\frac{45}{n}(T-H)$, in which T and H give the number of cloudy and clear days, including those in which the mean daily cloudiness is greater than 80 and less than 20 per cent. respectively. Applying this to the 13 forestry stations about Eberswald, the 110 German stations centering at Berlin, and the 73 Swiss stations, the differences between calculation and observation are found to be small, and are + and — in about equal number.

(2) KÖPPEN: Die Untersuchungen von Pater B. Viñes über westindische Orkane, p. 348–356.

The editor of the *Zeitschrift* calls attention to the work of Padre Benito Viñes, as yet hardly known in Europe, entiled "*Apuntes relativos a los huracanes de laz Antillas en setiembre y octubre de 1875 y 1876*" (Havana, 1877, sm. 4o, 256 p.), and gives an abstract of its results. American readers may have already seen a similar abstract in Ferrel's Meteorological Researches, part II, 1878, made in order to furnish data for comparison between theory and observation. The topics discussed are the radii of the different parts of the storm, the barometric depression,

the path and velocity of advance, the appearance of the different parts of the storm, and especially the signs of its coming. Köppen suggests that the occasional absence of heavy clouds in the immediate storm-centre may be due to the presence there of descending winds, but we do not understand by this that he discredits the general helical ascent of the winds around the centre; and he criticizes the opinion held by Viñes and others on the inclination of the cyclone's axis, which implies that the whole storm turned around on a single mathematical axial line. It is pointed out that the cirrus overflow of the tropical storms needs additional study.

(3) Fuess: Ueber zwei neue Registrirapparate für Windgeschwindigkeit und Windrichtung, p. 356–362 (from *Zeitschrift für Instrumentenkunde*, 1884, No. 9).

The author is an instrument-maker in Berlin, and describes the "Assmann" and the "Sprung-Fuess" anemographs, which give frequent record of wind velocity and direction; their cost is 630 and 1100 marks; an instrument of the latter pattern, in operation at Bucharest, gives the fullest satisfaction.

Correspondence and Notices—

(4) Zöppritz: Beiträge zur Kenntniss des Klimas von Darien, Chocó und Nicaragua, p. 361–366.

Brief description of climate in Central America from the observations of Selfridge, Lull & Collins, and Wyse.

(5) Zöppritz: Ueber die Abhängigkeit der Depression der Thermometer von der Zusammenzetzung des Glases, p. 366.

Confirming earlier observations that show that the continued depression of the readings of a thermometer, resulting from slow change in the glass, is greatest in glass containing about equal proportions of potash and soda.

(6) Friesenhof: Die Gezeitenlehre in der Wetterprognose, p. 367–371.

The author makes a comparison between the tides and the course of the weather, basing his work on the "law," announced by R. Falb of earthquake notoriety, that weather changes are dependent on the single factors of the tidal curve (such as distance, declination and configuration of moon and sun), and not directly on the total curve; his predictions thus attain a surprising success. Dr. Köppen appends a note to the effect that in phenomena so complicated as weather-changes, generatizations can be safely based only on accepted physical and mechanical laws; an empiric proof must rest on a great volume of material, and is not to be established either by the general impression of "surprising success," or by percentages that are taken independent of the law of probability.

(7) Krankenhagen: Zur Characteristik der dritten Mai-Pentade, p. 371–372.

On the recurrence of cool weather with frost, from the 11 to 15 of May.

(8) J. v. Bebber: Ueber die Fluth und Ebbe in der Atmosphäre, p. 373.

On the attempt of Russian observers to deduce tidal motions from hourly wind-records; the duration of the records is insufficient to establish any such effect.

Besides a few additional notes, there is an excellent current bibliography, continued from the earlier numbers of the *Zeitschrift*, in which a page and a half is given to this Journal. W. M. D.

Nature. A weekly illustrated journal of science, edited in London. Macmillan & Co., 112 Fourth ave., New York.

(9) Thompson, James: Whirlwinds and Waterspouts, vol. 30, No. 27, pp. 648–9.

The author briefly discusses: 1st. The retardation by friction of the gyratory motion of the surface air in

cyclones at sea, and the consequent inward and upward motion of this air; 2nd. The cause of the diminished pressure in the center of cyclones, and of the origin of cyclones; 3rd. The appearances and causes of waterspouts. The article is a brief presentation of a theory which has been very much more fully developed by Ferrel. The author, how ever, appears to think that only the surface air has a motion toward the centre of cyclones, but offers no explanation as to what force sustains the gyrations against the friction which, undoubtedly exists independent of contact with the earth's surface; he also seems to think that he was the first to suggest that the more direct influx of the air at the bottom of the cyclone was due to surface friction. His question, whether the decrease of pressure in cyclones may not be very much beyond the abatement that could be due to heat, or heat and moisture, alone would seem to indicate that he was not very familiar with the writings of Ferrel on this subject.

(10) Brodie, F. J: The rainfall of 1884, vol. 31, No. 3, p. 56–8. The author gives and discusses two tables. The first shows the excess or deficit of rainfall in percentages at various stations in England, Wales, Scotland and Ireland during each interval of three months, and during the whole of the first ten months of 1884. The second is for comparison, and shows "the total amounts of rain in London during some of the driest of the past seventy-one years, together with the percentage difference from an average based on the seventy years' observations; 1813 to 1882." A glance at table I shows that there was a decided deficit of rainfall from April to November in Great Britain, which was most marked in the vicinity of Oxford and London, at which places the rainfall was below the normal during every one of the first ten months of the year. Table II shows that the first ten months of 1884 were the driest on record at London. The deficit in 1847 was nearly as great being 37% while in 1884 it was 38%.

(11) Archibald: An account of some preliminary experiments with Biram's anemometers attached to kite-strings or wires. vol. 31, No. 3, p. 66–8.

The author's object has been to determine the law of the increase in velocity of the wind with the height, and he obtained measurements at various times of the velocities from the surface up to heights of about 1,000 ft. (1,500 above sea level). He describes his method thus:—"When the large kite is about 100 feet or so from the winder, and steady, an anemometer (Biram's) is fastened to the nearest 100-foot mark and its indicator, and the time noted. The wire is then payed out a certain distance, and another anemometer attached, and so on, the interval between the lowest instrument and the winder being regulated by whether the differences of velocity are required for a comparatively high or low altitude respectively. The altitudes are measured by taking the vertical angle of the instruments every ten minutes with a theodolite placed at the winder, and combining their average values for the whole period with the lengths up to each instruments."

He gives a table giving 23 sets of observations, every one of which shows a very considerable increase in the velocity of the wind with the height, but not with the rapidity which Stevenson's formula would give. He thinks he has obtained evidence that the velocity of the upper currents reach a minimum during the afternoon when the lower currents reach a maximum. He also thinks his kites have given evidence of an ascending current under cumulus

and cumulo-stratus clouds, and a descending current in their rear. He anticipates employing the same means of elevation for observations of temperature, pressure, height of cloud-strata etc., having received a government grant for that purpose. Such observations will, no doubt, be of exceptional value to meteorologists.

(12) HILDEBRANDSSON: The distribution of the meteorological elements in cyclones and anticyclones. vol. 31, No. 4, p. 75-5.

This is an editorial review of a paper presented by Hildebransson to the Royal Society of Science of Upsala in March, 1883. The editor summarizes our present knowledge on the subject as follows:—"In the front part of the depression the weather is warm, moist, and cloudy, whilst in the rear it is cold, dry and clear. Further inquiry showed equally distinct types of weather characterizing different parts of high pressure areas or anticyclones. So close indeed are these relations that the study of weather resolves itself into examination of the phenomena attending cyclones and anticyclones.

In Hildebransson's paper "the direction and velocity of the wind as noted at Upsala at the surface of the earth, in the region of the lower clouds, and in the region of the cirrus, the temperature of the air, the amount of cloud, the frequency of rain, the transparency of the air, and the occurrence of fog are examined with reference to forty-three different sections or areas into which the author has portioned cyclonic and anticyclonic systems. * * *"

Some of his leading results are:—

"From the central region of the anticyclone the velocity of the wind increases as the barometer falls, and the maximum velocity is reached on approaching the calm central region of the cyclone. With respect to the gradients, the greatest velocity occurs when the gradient is directed toward the north and the least when the gradient is toward the west or southwest."

"In the region of the lower clouds the wind takes a direction to the right of that of the wind at the surface of the earth. In the higher region of the cirrus clouds, the winds blow centrifugally from the region of the cyclone toward that of the anticyclone. The velocity is least in the vicinity of the central region of the cyclone, but it steadily increases as it approaches and flows over the region of the anticyclone."

In the winter it is warmer at the earth's surface in the cyclone than in the anticyclone; in the summer this is reversed. In winter the decrease of temperature with the height as shown by observations at the top and bottom of the Puy De Dome is much greater in cyclones than anticyclones. In anticyclones it is sometimes found warmer at the top than at the base. H. H. C.

BOOKS, ETC.

(13) **General Studies on Temperature.** *Temperature of the Atmosphere and of the Earth's Surface.* Professor William Ferrel. Professional Papers of the Signal Service, No. XIII; 4o; 69 pp.; Washington, 1884.

This is an attempt to reduce some aspects of the thorny subject of meteorological temperatures to mathematical forms, and we must confess to laying the memoir down after perusal with a feeling of disappointment. The subject is one of extreme difficulty, but Professor Ferrel's well known ability and the activity of several eminent investigators (Langley, Förster, Frölich, Vogel, Violle, Crova, Desains, etc.) made us take up the book with lively expectations that we would now have an analytical

clue through this labyrinth sufficiently close, at least, to guide the work of observers to the crucial points. Reading was not followed by conviction, for while the mathematical developments seemed in themselves as unexceptionable as Professor Ferrel's mathematics always is, the physical data involved sometimes appeared antiquated and unsatisfactory.

The first part of the memoir relates to the solar heat received by the earth, and its variations in time and latitude. So far as the developments here relate to the phenomena outside the atmosphere they make a series of formulæ and numerical determinations which may be useful to the student. But, whenever the author passes to the state of things in or at the bottom of the atmosphere, his results fail to convince the reader of their value. His deductions here are based on the familiar expression Ap^{ϵ}, where A is the solar constant, p the coefficient of absorption and ϵ the secant of the zenith distance of the sun. This is the old formula, but both theory and the latest observations show that p is a variable both as to the kind of radiation and the depth of the atmosphere traversed, and also that even if p is a constant, it does not equal sec. z. Again on p. 15 we are told that equation (22) may be shown to be correct by experiment. As it is a general harmonic form true for all periodic changes of the kind under discussion, it is of course correct, but we fail to see its relation to experiment or, without closer definition of the form of the functions involved, its practical value. Again on p. 17, where the author makes $b =$ the barometric pressure and then makes $m=bt$, he neglects one of the most efficient modifiers of solar radiation, viz., the solid particles floating in the air.

Similar reservations to our complete approval of the other parts of the memoir occur as we read. The second part relates to the conditions determining temperature, and here we have more generality given to Dulong and Pettit's law of cooling than physicists will now be disposed to admit. In the third part, relating to actinometry, the modern methods receive but little attention. The fourth part relates to the distribution and variations of temperature, and here we think the modifications of temperature due to currents of air and to the changes of state of water have a much greater importance than is given them.

It is easier to find fault than to improve, but the memoir of so eminent a student on so important a subject has this noteworthy difference from the writings of authors less eminent, that it carries so much weight with it as to cause its general acceptance, errors and all. It is therefore the duty of a critic to point out faults in eminent work when he might properly overlook the faults and see only the excellencies of the work of less known writers. To be criticized is one of the privileges of talent, and by pointing out what we believe to be serious defects of this memoir, we class the author among writers of talent. X.

(14) **Professional Papers of the Signal Service.** No. XIV. Charts of relative storm frequency for a portion of the northern hemisphere. By John P. Finley, Sergeant Signal Corps, U. S. A. Washington City, Signal Office, 1884. 4to; 13 folded charts.

We are glad to see that the over-cautious note which has prefaced the earlier numbers of the Professional Papers, disclaiming "indorsement of the views or theories set forth" therein, is omitted here. The publication of the author's name seems sufficient indication that upon him must rest the responsibility as

to him belongs the credit for the opinions and results in all work not simply mechanical or automatic.

Lieut. Finley has performed a valuable and laborious task in bringing together so large a share of the trustworthy data concerning the attitude of storm-centres, gathered in the last twenty years. His chief authorities are Leverrier (1864), Hoffmeyer (1873–'76), Mohn (1867, '68), Toynbee (Aug., 1873), Deutsche Seewarte (Jan.–Mar., 1878), and the great mass of material in our Signal Service records, both national and international. In the preparation of his extended series of maps, the tracks of "barometric minima" were first drawn from these authorities, and the number of cases in which storm-centres occurred within two and a half degree "squares" was then counted off for the several months of the successive years. These numbers were next summarized on an annual and twelve monthly charts, on which shades of color indicate the relative frequency of the passage of storms over North America, the North Atlantic and Europe.

A correction for the unequal areas of "squares" in different latitudes has been applied, as I am informed by the author, although no mention is made of it on the introductory pages. This must be regarded as a serious omission, for, while the colors on the charts may thus show correctly the relative storm frequency, there is no clue given to the absolute frequency. The key on the side of the annual chart, for example, indicates that, in the region of maximum frequency about our Great Lakes, twelve to fifteen storms occur annually on a $2\frac{1}{2}°$ square; but there is no statement of the latitude to which this standard square is referrred.

The earlier investigation of this subject was based on wind records, as in the storm charts by Maury and by Andrau of the Dutch Meteorological Institute at Utrecht (1862). In these, gales and not barometric minima form the basis for averaging; and this being the form especially adapted to the navigator's use, it is continued in the set of monthly Meteorological Charts, lately completed for the North Atlantic by our Hydrographic Office, on which the percentage of gales and squalls in the total number of wind observations for every five degree "square" is represented. The results as presented on these nautical charts and on Finley's maps are not closely comparable for several reasons. On the latter, it is not the gales, but the centre of their theoretic spirals that has been considered; and as in a given Atlantic storm there is a greater probability of gales in its southern than in its northern half, we should expect to find the greatest frequency of barometric minima farther north than that of stormy winds. Again, as the position of the storm centre is very seldom directly determined, and is in the great majority of cases discovered by inference from observations on wind and barometer at surrounding stations, charts of storm-centres can include a larger area and present a truer picture of the physical condition of the oceans than is seen on charts of gales, in which the results are to a greater extent subjective in being dependent in part on the routes of most frequent passage. And finally, another obstacle to making a comparison between charts of these two kinds lies in the lack of evidence to prove that all the barometric minima, whose tracks have been examined in the construction of Finley's maps, have been marked enough to give rise to storms in the seaman's sense of the word. This is evidently in part the result of the absence of

any accepted terminology. "Storm" and "barometric minimum" should not be considered synonymous; and charts of *storm* frequency should be based only on minima of sufficient relative depression to produce violent winds. In the present case, where such discrimination might have been difficult, a more non-committal title might have been chosen. Professor Abbe's map (pl. VI) of the "Frequency of Storm Centres" from the 100° meridian to the Atlantic, prepared for Walker's Statistical Atlas of the United States (1874) from two years of Signal Service data, is the first one of its kind so far as the writer knows. The maximum frequency was there clearly shown to follow the line of the Great Lakes and the St. Lawrence valley; but a region of numerous storms in eastern Nebraska, and an increase of frequency along the coast east of the Appalachians that were then determined, have disappeared in the average of a greater number of storms.

Following this by eight years comes Köppen's chart of average annual tracks and frequency of barometric minima between the Rocky and the Ural Mountains.* The representation of the commonly followed tracks here given in addition to the measure of frequency of occurrence is so great an advance on the previous maps and so essential an element in the practical and theoretical study of storms that we regret its omission from Lieut. Finley's monthly charts, especially as his introduction states that all the tracks have been drawn out in the construction of the charts as now published. As the paper is addressed to navigators, as well as to landsmen, it is evident that the direction in which a storm-centre will probably approach and pass away is quite as important as the probable frequency of occurrence. Perhaps the omission here may be remedied by the publication of a second paper as a later number of the series, in which the tracks would be the especial subject of study, and thus bring down to date and extend the monthly charts prepared for the United States by Lieut. Jackson, on the basis of three early years of Signal Service observation (Ann. Rep., 1874). The comparison of Köppen's map with Finley's annual average shows certain considerable differences: on the latter, the storm frequency about our lakes greatly exceeds that of any other region, while the former gives maxima on the far northern Atlantic and on the Baltic of as great a value as that adjoining our northern States. And the differentiation of grades over Europe on the former, which Köppen describes as the result of especial care, is much more detailed than in the latter.

Finley's advance consists chiefly in the use of a large quantity of material, the study of a very broad area and the publication of the results by months; and these elements are so important that they make his paper by far the most elaborate memoir on its subject that has yet appeared, in spite of the omission of the storm tracks. The addition of a boundary line enclosing the area studied would have been an improvement to the maps, by separating the regions demonstrably free from storms from those beyond the reach of observation.

The execution of the work by the Signal Office lithographers has not resulted well. The successive shades of blue chosen are not pleasing; corresponding grades of frequency are

* This appeared in the *Annalen der Hydrographie* and the Austrian *Zeitschrift für Meteorologie* for 1882, and again in different projection in the Atlas of the Atlantic Ocean, published by the Deutsche Seewarte at Hamburg in the same year.

by no means always of the same shade on different maps, and the registering of successive impressions is very unsuccessful. The latter inaccuracy is especially to be regretted, as it might so easily have been avoided by printing from a single color-stone, on which the shades were indicated by combinations of dots and lines, with a solid tint for the highest grade. It is somewhat disappointing not to have so large a piece of careful work carried to a finer completion; especially as it is issued from the office to which we must look for our best scientific and technical illustrations of meteorology. W. M. D.

(15) **Italian Thunderstorms.***

The thunder-storms of Upper Italy are of great importance because of the hail, which usually accompanies them. The Italian meteorologists have been led to give them much attention from the hope that they might be predictable and the publication cited is the third of a series. This number has followed its immediate predecessor only after considerable delay, the latter having appeared in 1880. In general, the studies are those of average thunder-storms in their relation to time, space, the meteorological elements and the cyclonic motions of the atmosphere. In the previous number, however, Schiaparelli had already noted, in a general way, the low-center's-south quadrant relation which American meteorologists have brought into such plain view within the last year. This part of the number before Engineer Pini and his attention is much more firmly fixed on the relations of local storms to the center of high than to that of low pressure. As this is a matter of great current interest, the following rather long translation of S. Pini's words may not be unwelcome. They are on page 82 of this memoir. "A simple inspection of the tables" (given in the memoir but not reproduced here) "confirms immediately the conclusion drawn by Prof. Schiaparelli for 1877 concerning the relation of the isobaric lines and the state of the atmosphere of upper Italy. The first result is the evident fact that during our thunder-storm weather of summer there usually exists isobaric type called *Atlantic*, by which is meant the presence of a high pressure over all or part of the western aspect of Europe and Africa from Morocco to England Usually the anticyclone is on the Iberio—Moroccan coast,—though sometimes in the Bay of Biscay, less often on the channel, Great Britain or the North Sea; when the anticyclone is in Northern Germany, thunder-storms in Italy, are exceptional or even contra indicated. The effects of this condition of the atmosphere were recognized from the study for 1877. They were a zigzaging of the isobar on the northern and southern declivities of the Alps, and the invasion of a mass of air urged on by the high Atlantic pressure with a free passage over the Gulf of Lyons to spread over the Mediterranean. If this causes a baglike extension by turning the isobars back, we have a greater or less thunder-weather, and this is much increased when Upper Italy is enclosed in a sort of funnel with the great end eastward. The greater the excess of pressure on the Atlantic, and nearer the approach of the high pressure to the Spanish-Moroccan coast, the greater is the tendency to thunder-storms. Moreover in the rare cases when the high pressure covers also the entire north-west face of Europe and a high also

* *Publicazione del reale Osservatorio di Brera in Milano, No. XVII. Sui Temporali osservati nell' Italia superiore durante l' Anno* 1878. *Relazioni di G. V. Schiaparelli, E. Pini e P. Frisiani, Milano*, 1884. 4.o; pp., 99: plates, 9.

exists on the African-Asiatic coasts, and (more, rarely over Russia, in this novel and characteristic situation we always have many thunder-storms."

The author finds this Atlantic type of high pressure by far the most favorable but defines another type also favorable when the high pressure is on the Barbary coast. He also finds occasional thunder-storms with a nearly uniform and quite high pressure occupying Southern Europe or even the whole of the continent. These latter storms are rare and sporadic. He concludes that low-centers passing over the Atlantic and Northern Europe have nothing to do with his storms.

In the numerous maps accompanying the memoir show frequent cases of the southern extension of the low pressure lying northward which American meteorologists have noted as a common feature in times of a local-storm actually in the United States. The baglike extension here is, however, generally extended more to the westward than with us, being usually directed southwest from the center, and sometimes nearly or quite westward. The storms then occur on the south side or in the Southeast quadrant.

The conclusions which S. Pini draws concerning the prospects of prediction of thunder-storms are unnecessarily pessimistic. He thinks that we cannot for a long time expect good predictions, and that then the predictions will be so general as to be of little practical benefit. The problem is doubtless a more difficult one in Upper Italy, between the Alps and the Appenines, but we may hope that on our great plains, and over the low eastern mountains the prediction of local storms will soon reach a practical and useful stage. H.

(16) Die Erde als Weltkörper, ihre Atmosphäre und Hydrosphäre. Von Dr. Julius Hann, Prag, 1884. 8vo; 209 pp., with cuts and plates.

The recent reprint from the latest edition of the *Allgemeine Erdkunde* by Hann, v. Hochstetter and Pokorny gives opportunity for mention of its precise and compact exposition of the more important subjects included under astronomical geography, meteorology and oceanography. The second part occupies, however, only 78 pages, and is therefore not to be compared in point of completeness with the author's *Klimatologie* that was published in 1883. The directness of statement that characterizes the bool kmay be illustrated by a few extracts; first on clouds: "The cause of the suspension of the ice-crystals and water-droplets of clouds in the atmosphere is found in their exceedingly small weight compared to their surface, and in the resistance which friction with the air exercises against their fall. Moreover, a cloud is not a fixed mass, but is rather the indication of a place in the atmosphere where continual condensation of vapor occurs." Clouds are said to consist of minute water-drops or of fine ice-needles, and no mention is made of the fantastic theory that attributes a vesicular form to cloud particles. The causes of the vertical decrease of temperature are ably condensed in half a page; they are stated to be the relative diathermance of the air and its consequent dependence chiefly on the earth's surface for its warmth; the mechanical cooling of an ascending, expanding current; the greater frequency of clouds at low levels; the small surface of elevated mountains and plateaus, and the rarity of the upper air into which they reach. Compare this with the vague opinions held at the beginning of the century—for example,

by DeLuc, who wrote that the sun could not be hot, because mountains which rose towards it were cold—and we may consider with some complacency the advance in our knowledge of terrestrial physics.

The difficult subject of the relation of air-pressure and wind is presented in the following order: First, if every level layer were of the same temperature throughout, there would be no motion because gravity would everywhere act normal to the isobaric surfaces. Second, the known excess of warmth in the torrid zone gives a slope to the isobaric surfaces, as is shown thus:

	S. America, equator.	N. America lat. 39°.
Pressure at sea level,	759^{mm}	767^{mm}
" "4060^{m}elev.,	471 "	458 "

The isobar of 759^{mm} must therefore descend towards the equator, while that of 471^{mm} descends towards the pole, and thus the primary circulation is established. Third, the direction of motion is affected by the earth's rotation, so as to appear oblique instead of meridional, and the low pressure about the poles is referred to the centrifugal force thus produced; but there is no mention made of the share that Ferrel shows the same force to have in the low pressure of the equator and the high pressure of the tropics, nor in Tracy's early demonstration (1843) of the deflective effect of the earth's rotation alluded to, although both of these statements seem commensurate in importance with many that are included in the book. There is, however, very seldom any call for such comment, and any minute shortcomings are far outweighed by the accuracy of the work as a whole, and by its being thoroughly brought up to date. W. M. D.

(17) **Elements of Forestry.** By Franklin B. Hough, Ph. D., Chief of Forestry Division, U. S. Department of Agriculture. Cincinnati; Robert Clark & Co. 1882.

The object of this work is most commendable, though it is questionable how far it has been attained. It is certainly high time for instruction in this subject to begin to take the place of agitation, as is manifest to anybody who will take the pains to look in upon a forestry congress and observe the proceedings of the gentlemen who have taken upon themselves to save the country. In turning over the book, however, one gets the impression that we are not yet, "quite out of the woods." There is still a very hazy light on the subject of climate and meteorological influences.

The old story of the ruin that is brought upon countries by the clearing off of woodlands needs revision, to say the least, in the light of what is now known respecting the climatic changes of later geological times.

The sequence of chapters in the book is somewhat puzzling. After the discussion of climatic influences, we are suddenly brought to a consideration of reproduction from seed. Then comes a technical chapter on planting and forest managing, while in the next succeeding one the author returns to the botanical aspect of the subject, and gives an account of the structure and function of leaves, and the structure of wood and bark, and pleasantly mingles in its closing page an account of the truffle industry of France, experimental researches on the pressure of sap, and a theory of autumnal colors.

There are several instances in which a lack of scientific accuracy is apparent. Cross-fertilization is confounded with hybridization, and unsymmetrical with imperfect flowers. Fruits and seeds are badly mixed, and we are taught that the "cotyle-

dons or germinal leaves seek the air and light, forming the *plumule* or stem of the plant"!

In later chapters really valuable information is given on the due proportion of woodlands, timber culture acts, European plans of forest management and related subjects.

The author deserves commendation for the vigor and constancy with which he has kept these very important question before the public.

It seems, however, that the whole matter of forestry in the United States will have to be got at in some other way. Experimental stations will have to be established and accurate data for planting obtained, commissioners composed of scientists instead of politicians will have to investigate the subject of rivers and forests at their head waters, and the whole matter of timber culture acts will have to be thoroughly overhauled. On the whole the time seems hardly to have come for teaching the elements of forestry in our schools, even with such a work as this for a text-book. X

(18) **Winslow Upton.** Report of observations made on the Expedition to Caroline Island to observe the Total Solar Eclipse of May 6, 1883, Reprinted from the memoirs of the National Academy of Sciences vol. II, 1884, 64 pages, separately paged.

In this are given the observations and reductions for the longitude of Caroline Island (occupying 18 pages) and the meteorological observations made while there, and the deductions from them. The general weather conditions during twelve days residence were warm and moist, cumulus cloud, and frequent slight showers. The barometer moved regularly over its diurnal change, having its maxima at 9 A. M. and 9 P. M., and its minima at 3 A. M. and 3 P. M. There were also indications of a general minimum on April 27, and a maximum on May 4, the latter being accompanied by the by the only rainstorm. Though the island is in the S. E. trades, the wind was always noted as E. or N. E. On April 30 there was a wind-squall without rain. During the eclipse there was a small but well marked rise in atmospheric pressure, a depression of temperature of about 4°, an increase of relative humidity of five per cent. and no appreciable change in the direction or force of the wind. The hourly speed of the ship, during the voyage from Callao over the constant conditions of the South Sea, always under sail, was studied with a reference to a diurnal change in the wind and it was found that the means for the speed showed a maximum for 11 A. M. and a minimum for 11 P. M. with unexpected regularity in the progression from one to the other. The solar radiation was studied with conjugate thermometers of the Marie Davy pattern with Violle's conjugate bulbs, and with two ordinary Green thermometers, one having its bulb blackened. The results for the solar intensity were quite variable, largely due to the capacities of the instruments, but by careful and judicious reduction they give the solar constant (amount of heat in calories received on a square centimeter outside the atmosphere) at 2.360 with fairly concordant individual results. During the eclipse, the thermometers lagged three or four minutes and showed no effects of heat received from the atmosphere. In the middle the temperature of night was attained. H.

(19) **Benito Viñes,** S. J. *Observaciones magneticas y Meteorológicas del Real Colegio de Belen de la Compañia de Jesus en la Habana, Año de 1875.* Havanna, 1884. 4to.

This publication is elegantly gotten up, and gives for each day of 1875 the maxima, minima, range and mean (for bi-hourly observations, except at night from 10 to 4) for the magnetic declination and horizontal force, barometer, thermometer, tension of vapor and relative humidity, the direction and maximum and minimum velocities of the wind and the amount of evaporation and precipitation. The data are also given by hours and for the month and year as a whole. The progress of the meteorological elements is also represented by attractive graphic designs on so large a scale that they can be read by hours. To each month are added notes on the synchronous changes of magnetic and meteorological elements. Sr. Viñes opinions on this subject have met with some discussion by Mr. Curtis in the first number of this JOURNAL. The appearance and convenience of handling of this report make it unusually attractive. H.

(20) **A. Kammermann.** *Résumé météorologique de l'année 1883 pour Geneve et le Grand Saint-Bernard.* From the *Archives de la bibliotheque universelle*, Oct. 1884, Geneva. 44 pages; separately paged.

This is the latest of the annual reports of the weather on Great St. Bernard as compared with that of of Geneva 2070 meters (6791 feet) lower. The observations were inaugurated in 1847 by Professor Plantamour and have been carried on on his plan until the close of this report (Dec. 1, 1883) from which time they will be slightly changed. The mean temperature on St. Bernard was 28°.5 F. The coldest day was Mar. 13, when the mean temperature was —4° F.; the warmest (56° F.) was Aug. 14. The variation of the meteorological elements from the means was similar to that at Geneva. H.

(21) **Dr. G. Hellmann.** Grösste Niederschlagsmengen in Deutschland, mit besonderer Berücksichtigung Norddeutschlands. Reprinted from the *Zeitschrift des königlich preussischen statistischen Bureaus*, Jahrgang 1884, Berlin, pp. 251–61.

This is a study of maximum rainfalls for the month day and hour in North Germany with abundant comparisons with those of South Germany and Austria-Hungary. It is a careful study from the statistical standpoint and includes in no cases less than five years' data while in many cases the series of observations extend to a quarter or half a century while they twice snrpass the latter limit (Tilsit 64 years, Danzig 54 years.) The author finds the results, in territory, time and average, remarkably uniform. The greatest rainfall for one day was at Ragusa on Dec. 13, 1872 when the depth of rain reached 298^{mm} (11.7 inches); the greatest actual fall in an hour was at Waltershausen Aug. 14, 1884, 75^{mm} (3.0 inches), but at Wermsdorf in Saxony 31.4^{mm} fell in 15 minutes on May 9, 1867, which is at the rate of 4.9 inches per hour. H.

(22) **A General Description of the State of Alabama,** with reference to the character of its various soils, the vegetables, arboreal and horticultural products, and its general agricultural advantages, together with a variety of information, upon subjects interesting to persons in search of new fields, of industry and enterprise, compiled by the department of agriculture, 1884. 8vo; 144 pages, with a map of the State.

The title of this excellent publication sufficiently indicates it character. It only remains to add that it has some notes on the climate of the State and a report on the average temperature and precipitation for thirteen years. It is indispensable

to any one wishing a knowledge of the present condition of Alabama.

(23) **F. Salino.** *Iolette, Monti e Caverne della Liguria.* Turine, 1884. From the *Bollettino del Club Alpino*, No. 50, 1883. 15 pages; 8vo, separately paged.

This reprint relates to a cave near Albegna, on the Ligurian coast, called the Grotto of Santa Lucia. It presents some peculiar features which are discussed. Though giving abundant signs of the action of water, it is now dry, due probably to the general drying of the mountain, slope, which in its turn is due to destructive agriculture. In addition to stalactitic structures, the cave has lateral calcareous shelves on which may be found salactites. These curious structures are connected with the fact that the cave is highest at its exit and must then have served as a catch basin for infiltrating water. Perennial and periodic springs would arise from such collections of water. H.

(24) **Tennessee Crop Report for October**, published by the Bureau of Agriculture. Reports from 42 stations with a map of the precipitations, isotherms and winds for the month. The mean temperature and precipitations of the month are given from 1871 for Knoxville, Chattanooga, Nashville and Memphis; the dates of the first and last frosts at Nashville are given for the same period, and for Riddleton for 1882-4, Ozone observations are now to be taken by many observers.

(25) **Daniel Draper, Ph. D.**, Director New York Meteorological Observatory of the Department of Public Parks, Central Park, New York, Abstract of Registers of Self-recording Instruments. October 1884, 4 p.

(26) **Charles Carpmael,** Superintendent Monthly Weather Review, Meteorological Service. Dominion of Canada, October 1884, 8 p. On October 1st there was a magnetic storm and an aurora. On the other dates when the needles were disturbed no auroras were seen but on two of them the sky was clouded.

(27) **Michigan Crop Report No. 37,** November 1, 1884, 8pp. Met. summaries from 29 stations.

(28) **Report of the Ohio Met. Bureau** for the month of October, 50 pp. Reports from stations with chart of rainfall.

CORRESPONDENCE.

THE TWILIGHT DISCUSSION.

TO THE EDITOR:—It is evident that Mr. Schaeberle, in his communication to your JOURNAL for November, has overlooked the most important and remarkable circumstance of my twilight observation to which I called particular attention; and he has certainly offered no explanation, that will dispel the mystery in which the chief feature of the phenomenon I described is still involved. It was the *pyramidal form* of the illumination that appeared to be the most noticeable feature of the phenomenon, and which principally induced me to describe my observation in the October number of this JOURNAL Mr. S. refers only to my query as to the possible connection of the phenomenon with the zodiacal light, but its most surprising feature receives no attention from him. My query had no reference whatever to the physical connection of the two phenomena, and it is perhaps needless to inform your readers that the query was suggested by the *similarity of form only.*

leading me to imagine that both may be produced by the same cause—if the illumination was not an atmospheric phenomenon—without being necessarily associated physically, or otherwise connected, except in appearance, which was particularly noticeable, and naturally suggested a striking resemblance, if not a peculiar association, at least to the extent that their origin might be somewhat identical.

It is true as stated by Mr. S.—and I was well aware of the fact—that the zodiacal light is always "fixed with reference to the stars," and has an apparent diurnal motion, from east to west, rising, passing the meridian, and setting, as does the Milky Way and the radiant point in meteoric showers. I was also acquainted with the fact that the zodiacal light "always lies along the ecliptic"—the axis of the former being nearly coincident with the latter,—which accounts for the nonappearance of the phenomenon at the time of my observation, and, in fact, throughout the autumnal and summer months, except in the east before sunrise. As the zodiacal light is never visible in the west at the season of my observation, owing to the small angle of its axis with the horizon, I understood very well that its position did not correspond with that of the illumination I described, at the time I witnessed the phenomenon. The peculiar illumination resembled in form the vertical section of a cone, or pyramid, with its axis nearly straight angles to the horizon; and,—as I stated in my description of the phenomenon,—its position, form, and extent were almost identical with the similar features of the zodiacal light, *as it appears in March*, when generally the most conspicuous, my statement having no reference to the position or appearance of the zodiacal phenomenon, as compared with the twilight illumination, *at the time of my observation*, where the form was *the only feature* that suggested a resemblance or connection between the two phenomena.

That the phenomenon was indeed "a fine example of our sunsets," certainly no one would dispute who was fortunate enough to witness it; but the illumination was so remarkable, and anomalous in its appearance, as to suggest other causes than those by which ordinary atmospheric phenomena are explained. Since the publication of my description in this JOURNAL, I have myself arrived at the conclusion that the phenomenon,—however it may be explained,—was of atmospheric origin, as Mr. S. also maintains, though such a concession does not preclude the possibility of its cause being identical with that which may produce the zodiacal light, or even the solar corona, when we consider that their origin is still an open question, and that many astronomers no longer regard them as solar appendages, but concur in the opinion that both must be attributed to other causes than those by which they have hitherto been explained. The solar corona, particularly, has received a new explanation of late, and recent discoveries appear to decide that the corona is certainly not a solar appendage, but simply a diffraction phenomenon.

Mr. S. says that "the meridian distance of the ecliptic at time of observation was more than 60°;" but the statement is so obscure that I am unable to interpret its meaning.

If any of your readers will examine a celestial globe, rectified for the time of my observation, (Sept. 11, 6:30 P. M.) or—what I consider still better—Whitall's "Movable Planisphere,' the position of the ecliptic, relative to the meridian and horizon, may be very easily ascertained, when it will be found that the ecliptic,— a small arc of which may be regarded as a straight line upon the sky —intersects the meridian almost at right angles, and that the autumnal equinox is just above the western horizon. Owing to the small angle at which the ecliptic is inclined to the horizon, the zodiacal light would necessarily be invisible at the time of my observation.

Now, as to the general phenomena of our red sunsets, while it appears to be a decided conclusion among those most eminently qualified to express an opinion, that the colored appearance of the sky is produced by the matter in the upper atmosphere, that the important problem that still awaits the solution,—and only one concerning which there are differences of opinions,—is the nature of the suspended matter causing the observed phenomena. An interesting question in connection with the one above mentioned and which has received but little attention, relates to the mysterious appearances, and anomalous features, (such as the pyramidal phenomenon I described,) that are witnessed in our twilight exhibitions, and which certainly call for some explanation. It is evident that the uncouth forms, observed in the sunset phenomena, and the variety of positions which they appear to assume, are not explicable by the familiar laws of optics applied to ordinary appearances of the

I have never observed the phenomenon in the eastern heavens, to which Mr. S. refers, though I have often seen the dark and illuminated bands mentioned by him radiating from the horizon after sunset. These phenomena are of frequent occurrence, particularly in summer, and admit of an easy explanation, as does also the phenomenon, known by the name of "the sun's drawing water,"—which they much resemble,—produced by the shadows of clouds seen against an atmospheric haze, the apparent divergence being only an effect of perspective. The phenomena of dark and illuminated bands are not peculiar to the evening twilight, but are frequently witnessed in the morning just before sunrise and are believed to explain that ancient poetic expression, "the rosy fingered morn."

The subject of our recent twilight phenomena is worthy of particular attention from meteorologists, and more careful investigation may not only throw some light upon the remarkable phenomenon I have described, but open to our astonished contemplation a variety of cosmical or atmospheric matter with which we were previously unacquainted.

Very Truly Yours,

ARTHUR K. BARTLETT.

BATTLE CREEK, MICH., Nov. 27th, 1884.

MR. SHERMAN'S METHOD OF WEATHER PREDICTION.

TO THE EDITOR:—I was most exceedingly interested in the article in the Nov. METEOROLOGICAL JOURNAL in the possible relations of Dakota to Michigan weather, and if the conditions supposed to exist in that article can be proved to be actual and the relations between different points established so that they can be easily understood, it would be of more *practical advantage* to all engaged in *Agriculture* than any and all the previous developments of the science. It is of comparatively *little* advantage to the planter to know 24 or 36 hours in advance of a storm which may be wind *or* rain, even if the knowledge could be absolute and complete; but, it would be of *immense* advantage to him to be able to tell with a reasonable degree of certainty what the weather (as to local rains and temperature) would be, two or three weeks in advance. For instance, if in the early spring of 1882, the Michigan farmer could have known that the comparative dry weather of those days was to be followed by the almost incessant local rains of the late spring, it would have enabled him to have pushed his preparation and planting (which, in *ordinary seasons* it is better to drop until later,) so that the plants could have been well established and enabled to stand the wet weather which followed. Again the result in many crops is almost dependant upon the weather for the *first two or three weeks after sowing,* and if this could be known, many a farmer would be able to substitute some other crop for one which is certain to be comparatively a failure. So, too, the treatment of a

crop already under way would be in many cases most materially modified if one could know what conditions were to follow. In short, such knowledge would almost insure success in agricultural operations, for I cannot remember a success, in which, if I had known just (or even approximately) what the weather would be two or three weeks in advance, I could not have turned most of its failures into success. So far all the work in this direction has been of service to the sailor and the shipper and dealer in produce rather than to the farmer, who, as I said, cares much less to know precisely what the weather will be to-morrow, than to know approximately what it will be for the next 10 days. But it seems to me that it can hardly be possible (in view to the importance of the matter) that if such relation exists, it has not been pointed out, for it seems that a careful study of reports, from stations not far east or west of each other, would demonstrate the matter. I am sorry that I cannot take time to present this matter through your JOURNAL, but I cannot take the time necessary, and I only write to urge that you call the attention of some competent person to it, for the possible good results are so great, that it ought to receive the most careful attention. I would say, too, that all of my study and reading of weather reports from day to day has been far more to enable me to make a *guess* at what we are *likely* to have for a month ahead, than to *know* what it was to be the next 24 hours, and I know that this is true of *many of our most successful agriculturists* and that they with me would be most *intensely* interested in investigations which might enable them to do this more accurately.

Yours Truly,

WILL W. TRACY.

DETROIT, Dec. 1884.

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